

Researches on the effects of bloodletting in some inflammatory diseases : and on the influence of tartarized antimony and vesication in pneumonitis / by P. Ch. A. Louis ; translated by C.G. Putnam ; with preface and appendix by James Jackson.

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*Kind remembrance
of Dr. Peirson*

Salem, May 24. 1836.

RESEARCHES
ON THE
EFFECTS OF BLOODLETTING
IN SOME
INFLAMMATORY DISEASES,
AND ON THE
INFLUENCE OF TARTARIZED ANTIMONY
AND VESICATION IN PNEUMONITIS.

By P. CH. A. LOUIS,

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the Royal Academy of Medicine of Paris, Corresponding Member of the Medical Society of
Marseilles, of the Imperial Medico-Chirurgical Society of St. Petersburg and of
the Medical Society of Edinburgh; Member of the Legion of Honor.

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TRANSLATED

By C. G. PUTNAM, M. D.

WITH PREFACE AND APPENDIX

By JAMES JACKSON, M. D.

Physician of the Massachusetts General Hospital.



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1871

PREFACE BY J. JACKSON, M. D.

THIS work has been translated by Dr. Putnam, at my suggestion and request, and it has been my part to add to it a preface and an appendix.

It is the latest of Mr. Louis's publications, or the latest which has reached us. It differs from his other works ; for they related principally to the pathology, to the diagnosis and the interpretation of symptoms, in the diseases of which they treated. This relates to therapeutics, and principally to one of the most interesting questions in this branch of medical science. If any thing may be regarded as settled in the treatment of diseases, it is that bloodletting is useful in the class of diseases called inflammatory ; and especially in inflammations of the thoracic viscera. To the general opinion, or belief on this subject,

M. Louis gives support by his observations; but the result of these observations is that the benefits derived from bleeding in the diseases, which he has here examined, are not so great and striking, as they have been represented by many teachers. If the same results should be obtained by others, after making observations as rigorous as those of M. Louis, many of us will be compelled to modify our former opinions. But whatever may be the conclusions, in which we may ultimately rest, this work must be regarded as highly valuable. The author does not pretend that the questions, here discussed, are decided for ever. He makes a valuable contribution to the evidence, on which they must be decided; he points out the mode, in which this evidence should be collected, and in which its materials should be analyzed; and, seeking truth only, he calls on others to adduce facts, which, being gathered from various quarters, may show us, with a good degree of exactness, the precise value of the remedy in question.

It should be kept in mind, however, that our labors in this cause are not to be performed carelessly. It is in proportion as our observa-

tions are made with exactness and discrimination, that they will satisfy our own minds; and in proportion to the evidence of our care and accuracy that we can satisfy others. First, we must be careful as to our diagnosis; and second, we must be accurate as to the period of disease; third, we must be minute in noting the particulars, in which amendment is produced; and fourth, we must be precise in stating the extent and the manner, in which the remedy is employed.

To many of our readers M. Louis is not yet known. It would be sufficient to refer them to the pages of the work, which follows, for evidence of his fitness for the task he has undertaken. But, as it may be gratifying to many persons to know more of him, and of the method, which he has pursued in the study of medicine, I shall here copy a note respecting him, which I have printed in another work.

“P. Ch. A. Louis, physician of the Hospital de la Pitié, is a man, whose labors and whose writings must become more and more known for ages. I should deem it service enough to my brethren in this country, if I could induce them,

one and all, to read and study the works of this great pathologist. M. Louis is the founder of the numerical system, as it has been denominated, in respect to the science of medicine. It is the object of this note to state what that system is, and briefly to advert to the successful application of it by its founder.

“How many will be ready to turn aside, when they hear of a new system. Has not system followed system, it will be asked, ever since the days of the four humors. Facts, it will be added, observations, exact observations are wanting, not systems, in order to carry forward the science of medicine. Be it so; it is the last point, on which I would disagree with my reader. If however that reader has not had much experience on the subject, he may not be aware of the difficulty of making good observations, as regards both pathology and therapeutics, and of the caution which is requisite in making deductions. These difficulties should not deter us from adopting the right course; they should only make us study to find out what this course is. M. Louis certainly will not direct us to turn from observation to speculation.

“But to remove the objection, let me say at once that M. Louis has not brought forward a new system of medicine; he has only proposed and pursued a *new method* in prosecuting the study of medicine. This is nothing else than the method of induction, the method of Bacon, so much vaunted and yet so little regarded. But, if so, where is the novelty? If any one, after patiently studying and practising the method proposed by M. Louis, denies the novelty of it, I will not dispute with him a moment. Perhaps he will then agree with me that it is a novelty to pursue the method of Bacon thoroughly and truly in the study of medicine; though it is not new to talk of it and to laud it.

“A little history of one part of M. Louis’s life will throw some light on this subject. This gentleman went abroad, and I believe had some appointment in Russia, after he had gone through the usual course of professional education. Returning to France at the age of thirty-two, he was about to engage in private practice. He was then led to examine anew the state of the science of medicine, and was dissatisfied with it. He now decided to abandon the thoughts of prac-

tice for a time, and to devote himself to observation ; that is, to the study of disease as it actually presents itself. With this view he went into the hospital la Charité in Paris, and followed the practice of M. Chomel, now a physician at the Hotel Dieu and Professor of Clinical medicine, and highly esteemed as an author. M. Louis passed nearly seven years in studying medicine in this way. The first part of this time he was learning how to make observations. When he thought he had attained this art, he threw away, as I have understood, the notes he had already collected, and began anew to accumulate exact observations of the phenomena presented by the sick and of those derived from an examination after death in the fatal cases. In this course of observations he did not make a selection of cases, but took them as they were presented, indiscriminately. He was not in a hurry to make deductions from his cases, satisfied that he was gathering the materials, from which truth must ultimately be elicited. He was only careful that his observations should be correct, and had not any general principles, or doctrines, for which he sought support, or confirmation.

“To estimate the value of his observations, it is necessary to understand the plan, on which he collected them. First, then, he ascertained when the patient under his examination began to be diseased. Not satisfied with vague answers, he went back to the period, when the patient enjoyed his usual health; and he also endeavored to learn whether that usual health had been firm, or in any respect infirm. He noted also the age, occupation, residence, and manner of living of the patient; likewise any accidents which had occurred, and which might have influenced the disease then affecting him. He ascertained also, as much as possible, the diseases which had occurred in the family of his patient. Secondly, he inquired into the present disease, ascertaining not only what symptoms had marked its commencement, but those which had been subsequently developed and the order of their occurrence; and recording those, which might not seem to be connected with the principal disease, as well as those which were so connected; also, measuring the degree or violence of each symptom, with as much accuracy as the case would admit. Thirdly, he noted

the actual phenomena present at his examination, depending for this not only on the statement of the patient, but on his own senses, his eyes, his ears and his hands. Under this and the preceding head he was not satisfied with noting the functions, in which the patient complained of disorder, but examined carefully as to all the functions, recording their state as being healthy or otherwise, and even noticing the absence of symptoms, which might bear on the diagnosis. Thus all secondary diseases, and those, which accidentally co-existed with the principal malady, were brought under his view. Fourthly, he continued to watch his patient from day to day, carefully recording all the changes, which occurred in him till his restoration to health, or his decease. Fifthly, in the fatal cases he exercised the same scrupulous care in examining the dead, as he had in regard to the living subject. Prepared by a minute acquaintance with anatomy, and familiar with the changes wrought by disease, he looked not only at the parts where the principal disorder was manifested, but at all the organs. His notes did not state opinions, but facts. He recorded in regard

to each part, which was not quite healthy in its appearance, the changes in color, consistence, firmness, thickness, &c.; not contenting himself with saying that a part was inflamed, or was cancerous, or with the use of any general, but indefinite terms.

“Without presuming that I have described in the most exact manner the course pursued by M. Louis, I have said enough to make his plan intelligible to men of sagacity. Others have taken down cases in like manner. In the first volume of the “Transactions of a Society for the improvement of Medical and Chirurgical knowledge,” published 1793, there is a paper by Dr. George Fordyce, entitled, “an attempt to improve the evidence of medicine.” In this paper Dr. Fordyce recommends the careful collection of cases, as the only foundation for the improvement which he wished to see. Dr. Fordyce goes into many details, and gives two cases in a tabular form by way of illustration, and states that he has many cases collected upon this plan. In his plan some matters are insisted upon more than by M. Louis perhaps; others less. But Dr. Fordyce does not insist upon the examination

after death, a most important part of the plan adopted by M. Louis. If however the attempt proposed had been followed by vigorous efforts, most important benefits would have resulted from it. Many no doubt thought of doing it. I myself thought seriously of it more than thirty years ago, and had blanks printed for my cases, according to the plan of Fordyce. But the difficulties attending the plan in private practice discouraged me too soon. So far as I have known, M. Louis is the only physician who has devoted himself for years together, at a mature age and after a sufficient education, to simple observation, without the distraction of medical practice, and without having any share in the treatment of the cases under his observation.

“It was only when he had accumulated a great mass of cases, that M. Louis began to deduce from them any general principles. He then arranged the facts he had collected in a tabular form, so as to facilitate a comparison of them. How much labor this required will be in some measure conceived, when I state that, while going through one class of his observations, those, I believe, which relate to acute diseases, he re-

tired to a distance from Paris and occupied ten months in making out his tables. This statement is, I believe, substantially, if not precisely correct.

“Let the reader conceive of these tables drawn out with accuracy, having columns devoted, with proper discrimination, to each function and to its various derangements, as manifested during life, and to each organ and its lesions as ascertained after death; let him then go to these tables and inquire, under what circumstances certain signs of disease arise, and with what pathological changes in the dead body they are found to correspond; let him ask under what circumstances certain morbid changes of structure occur, and with what symptoms they are found to be connected; he may find the answers and he may obtain them numerically. That is, he may learn in how many cases out of a hundred of any particular disease he will find a certain derangement of a particular function, or a certain change in structure of a particular organ; and he may also learn how often the same things may be noticed in other diseases, with which that under consideration may be compared. For instance,

does he ask how often does it happen that dysphagia occurs in typhus fever? M. Louis replies from his tables that in the fatal cases of typhus, which he had examined when his work on this subject was written, rather more than one in five had this symptom. Is it then asked whether this symptom was found to be connected with any particular organic lesion, M. Louis says that in four out of five of these cases there were ulcers in the pharynx or œsophagus, or other change of structure in the organs concerned in deglutition. Thus it was shown, that it has been for want of examination that we have so often attributed this symptom to weakness. If the same questions are asked in respect to other acute diseases, the answer furnished by M. Louis is, that in the acute diseases, of which he had accurate notes, exclusive of typhus, severe dysphagia did not exist; and that ulcers were also wanting in the pharynx, &c.; though slight organic affections were found in a very small proportion of those cases.

“ Or, again, is it asked, how often the epiglottis, larynx and trachea are ulcerated in pulmonary consumption? M. Louis replies that the trachea

exhibits ulcerations in nearly one third of the subjects of this disease ; the larynx in a little more, and the epiglottis in a little less than a fifth of those subjects. Meanwhile, in other chronic diseases, M. Louis had found only one case, in which these parts were ulcerated, while the lungs were not tuberculous. The symptoms, by which the ulceration of the epiglottis was marked, were a fixed pain in the upper part of the thyroid cartilage, or just above it, and a difficulty of deglutition, such that the drinks are sometimes thrown out by the nose. The symptoms attending the other lesions are much less distinct, and the statement of them would lead to details not necessary in this place.

“ The instances here taken are the first which came to hand, on opening M. Louis’s publications ; but in like manner we may find an answer to most of the questions, which would arise in reference to the subjects discussed.

“ The experience of one man is necessarily limited, and more extensive researches may give results different from those at which M. Louis has arrived. But I am disposed to think that the difference will not be material in many

instances. His observations were made only in the hospitals of Paris. Other observations, made in different climates and among persons of different habits, will probably discover differences of some kind, and perhaps some which are material. But in most respects, since the works of M. Louis have been known to me, I have found his observations confirmed by my experience here; and indeed in many respects they accord with my own previous observations, being, however, more precise than mine had been.

“But there are various points in pathology, on which M. Louis has taught us what we did not know before. For instance, he has given us certain signs, by which we may recognise pericarditis, in a large proportion of the cases in which it occurs. He has shown that tubercles are always found in the lungs, if they are found in any other part of the body; or that the exceptions are so rare as not to be practically important. He has shown that chronic peritonitis is found only in tuberculous subjects. Others had suspected that the fever, now commonly called typhus, was dependent on inflammation of the mucous membrane of the stomach and

bowels; and it had been shown that in certain seasons the elliptical patches, called Peyer's glands, were the seat of inflammation, and commonly of ulceration, &c. M. Louis has shown that a morbid alteration of these glands is constant in typhus; constituting, as he terms it, the anatomical character of that disease. He has likewise pointed out the other anatomical changes, which belong to, and those which often attend the same disease; as well as some symptoms which had been overlooked, or not duly regarded by others. At present I can say that his observations, in regard to typhus, have been confirmed by all that I have been able to learn respecting it in this country, since his observations have been known to me.

“I am not, however, engaged in reviewing the works of M. Louis. I have not guarded myself in all points in stating his observations. I wish to induce others to read his books, and they will then see the prudent caution, with which he offers all general remarks, and the scrupulous care which he exercises in making his deductions. He studies nature with a full faith in the uniformity of her laws, and in the

certainly that truth may be ascertained by diligent labor. It is truth only he loves; not anxious to build up a system, nor pretending to explain every thing, he says to his pupils, such and such have been my observations; you can observe as well as I, if you will study the art of observation, and if you will come to it with an honest mind, and be faithful in noting all which you discover, and not merely the things which are interesting at the moment, or those which support a favorite dogma; I state to you the laws of nature as they appear to me; if true, your observations will confirm them; if not true, they will refute them; I shall be content if only the truth be ascertained.

“I wish to add that M. Louis has inspired a gallant band with his spirit. They have combined to form the Society of Medical Observation at Paris; M. Louis is their President and MM. Chomel and Andral are the Vice-Presidents. They meet to report their observations and to be corrected by each other and by their president, when their observations are inaccurate or deficient, or when their inferences are broader than their premises. The members are

selected without reference to their country; they are from different nations; they are scattered, and will in succession be scattered over the world; and all, who carry with them the true spirit, must contribute to the advancement of real science. Men who devote themselves thoroughly to labor, in whatever department, must be felt and known in society. Let the members of this society go on and throw the fruits of their labor into a common stock, and they must all of them be enriched, and all around them be enriched at the same time.

“To the remarks, perhaps too desultory, which I have given in this article, I am desirous to add two more.

“The two great works of M. Louis, which have yet been published, are that on phthisis and that on typhus. My first remark is, that the information given in these works is much less limited, than their titles would indicate. In the first, other chronic diseases are compared with phthisis in respect to its symptoms and to organic lesions; and thus it may be regarded as treating, to a certain extent, of chronic diseases, and embraces a vast deal of information

in respect to them. In the second, acute diseases are compared with typhus, with the like advantages.

“My second remark is, that the general results, to which M. Louis has attained by his mode of studying diseases, have been greater, that is, more numerous and more important, than might have been anticipated in so short a time. I think he could hardly have hoped for such abundant fruits of his labors, great as they were; diligently and faithfully as they were pursued. In this there is much encouragement. Already in his hands medicine, at least what regards the signs of diseases and the pathological states on which they depend, begins to assume the form of an exact science. In moulding his materials indeed, he has availed himself of the useful instructions which have been furnished by others, on whom he could depend. This is especially true in respect to the discoveries of the illustrious Laennec, to whose merits he renders ample justice. His own merit however is peculiar. While all were ready to acknowledge, that it is only by a careful observation of nature we can ascertain her laws, he only has had the

boldness and the vigor to undertake and carry through a series of full and exact observations, without prejudice, and with a determination to report his discoveries fairly and exactly, not magnifying, nor diminishing the evidence in any case to make it quadrate with principles previously imagined. One, who knew him intimately, bore this testimony of him, that he would not be tempted to entertain an hypothesis in any case, saying that it had almost uniformly happened that rigorous observation had refuted the hypotheses he had formed.

“I venerate M. Louis greatly. But it is not with the vain hope, nor even the desire to promote his fame by my feeble commendations, that I have written this note. I regard it as certain that his fame, and what he will regard much more, the truths which he has discovered, will be extended and will live for ages. My sole expectation is to lead some, who might otherwise be ignorant of them, among my brethren of the present day, to study works which I esteem as among the most valuable certainly, if not the most valuable, which any age has furnished us in regard to medicine. Unlike the systems, which

are always spoken of in the history of medicine, as successively rising with splendor and falling into oblivion, the principles published by the founder of the numerical system are not an artificial network, where the cutting of one thread may cause the whole to drop away ; these principles may be added to, they may be enlarged, limited and modified, and yet the system may be maintained ; and it will still derive its support from the first labors devoted to its erection as much as from the last. If, for instance, M. Louis has observed a certain symptom, such as the enlargement in the region of the spleen, to be present in forty-five out of fifty cases of typhus ; the exceptions will be ten per cent. Should subsequent observers find, that in a hundred and fifty cases there have been twenty exceptions, it will then appear that these in the two hundred amount to twelve and a half per cent. As far as I know, there are very few of M. Louis's numerical inferences, which have hitherto required to be modified so much as in the instance here supposed, since the publication of his great works ; although ten years have now elapsed since that on phthisis, and six years since

that on typhus was published ; and although he himself has continued, during this period, to devote a great portion of his time to the collection of new observations. Were it otherwise, however, it would be glory enough for one man to have led the way into the true path, and to have inspired others with the courage to follow him. I repeat the idea ; — it is the spirit of bold and hardy enterprise, which is the glory of M. Louis.

“I must add a few remarks on another point. It is objected by some to the labors of M. Louis, and of others of the French pathologists, that they labor indeed with ardor on the subject of diagnosis, that they study with the zeal of entomologists to discriminate minute changes of structure in the various textures of the human body, but that they do nothing to advance the proper business of the physician, the art of healing. Their therapeutics are decried, as showing an ignorance of what has been thought certain in England and in this country ; and they themselves are regarded even as indifferent to this branch of science. Can this objection need a reply ? I have long been satisfied, for thirty years I have been satisfied, that the physicians

of Paris were laying the firmest foundation for the science of therapeutics, by studying the natural history of diseases; and by thus giving us rules for diagnosis and prognosis. The course they have pursued has not always been the most satisfactory, and one at least among them has gone over to the dogmatic philosophers, though he has tried to disguise his desertion of the true cause. But the course they have pursued has led honest spirits to be more and more exact in their observations, until now, when one has arisen, who has vigorously undertaken all the toils, to which the method previously adopted would rightly lead them. Let them proceed in the same spirit, aided, but without any spirit of rivalry, by the pathologists of other countries; let us all learn what may be looked for, when art does not interfere in the diseases of the human body; that is, let us study the rules of prognosis, which are only inferences from the natural history of diseases; then we shall be prepared to study therapeutics. Let M. Louis, or men like him, test the effect of remedies in the same spirit, with which he has pursued his pathological researches. Having determined the average du-

ration, fatality, &c., of typhus, for example, by an observation of a sufficient number of cases through a series of years, such cases not having been actively treated, let him then employ in the same disease the different remedies which have been thought useful. One physician extols the advantages of bleeding ; another commends antimonials employed on the first days of the disease, in emetic doses, and for a few days afterwards in doses just short of nauseating ; another contends that cinchona is the best antidote to the deadly tendencies of this malady. Let each mode of treatment have its fair trial ; and let the results be compared with each other, and with similar cases, treated at the same time upon the expectant method.

“ This is substantially the mode in which questions in therapeutics are beginning to be treated in Paris. So, no doubt, they have been treated elsewhere. But it is in proportion as we arrive at precision, in respect to the natural history of diseases, that this mode will be pursued with the greatest advantage. It is because we are approaching to that precision that I think it scarcely rash to predict, that in fifty years the art of heal-

ing will be grounded on many exact rules, which we and our predecessors have not known. These rules will not be brought forward as derived from grand principles of physiology, or pathology ; they must be deduced from the aggregate of careful, faithful observations of individual facts, made by men of enlightened minds. A love of truth, an unflinching love of truth is the first requisite in those, who engage in this holy calling."

It will be seen that in this note I anticipated that great advantages would be obtained by M. Louis and his disciples, whenever they should turn their attention to the treatment of diseases. I did not then know, nor did I suspect, that while I was writing that note, M. Louis was already engaged in his first work expressly on therapeutics. The few pages indeed, which constitute the first chapter of this work, had already been published by him, and they had not escaped my notice. But it will be plain that, when alone, these pages could not make so deep an impression, as does the whole work here presented.

TO
MARSHALL HALL, M.D.

F.R.S. L. and E., etc.

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ADVERTISEMENT BY THE AUTHOR.

I PUBLISHED in the month of November, 1828, in the *Archives Générales de Médecine*, a memoir on the effects of bloodletting in some inflammatory diseases. This memoir was very differently received. Some, in consequence of prejudices, difficult of explanation, declared that I rejected bloodletting in the treatment of cases of inflammation, although I show the necessity of having recourse to it, in severe cases, for two cogent reasons. Others were surprised, undoubtedly, by the extreme difference which exists between the results, to which I have been brought, and the opinions most commonly received concerning the power of bloodletting; and these declared against the method, which I had pursued with a view to arrive at general princi-

ples, and in favor of that which is commonly called the experience of ages. Some physicians received my work favorably, being persuaded that the method which had been my guide, would necessarily lead to precise results in therapeutics. However, some copies of this memoir, having been separately struck off, were quickly sold; and my publisher, M. Ballière, urged me some time since to prepare a new edition. I thought proper to comply with his request; and this is the memoir in question, as it was published in the *Archives*, excepting some alterations in the style; and I now submit it anew to the judgment of the reader. I have added the analysis of some facts since collected, similar to those in my first publication; and by the aid of this analysis their value will be the more readily appreciated. Finally, to this analysis succeed an examination of the method which I have followed, and some remarks upon a few works on bloodletting.

The memoir published in the *Archives*, the analysis of the new facts, the examination of the method I have pursued in my researches, and the remarks above mentioned, will be the subjects of as many chapters.

RESEARCHES
ON THE
EFFECTS OF BLOODLETTING
IN
SOME INFLAMMATORY DISEASES.

CHAPTER FIRST.

Researches on the effects of bloodletting in some inflammatory diseases.

THE results of my researches on the effects of bloodletting in inflammation, are so little in accordance with the general opinion, that it is not without a degree of hesitation I have decided to publish them. After having analyzed the facts, which relate to them, for the first time, I thought myself deceived, and began my work anew ; but having again from this new analysis, obtained the same results, I could no longer doubt their correctness ; and I shall state them to the reader as they at first presented themselves to me.

These results without doubt will be far from satisfactory ; but of what consequence is that, if they are true ; since, whatever has this character, cannot fail in the end to be of real utility.

It may be proper to remark further that the facts, which I have collected, are neither so numerous, nor so varied, that the results can be considered henceforth as established laws : and my object in publishing them, has been chiefly to excite anew the attention of observers upon the effects of bloodletting in the treatment of inflammation.

Pleuropneumonia, Erysipelas of the face, and Angina tonsillaris, being the inflammations which I have observed the most frequently, must alone be the subjects of these researches.

ARTICLE FIRST.

Effect of bloodletting in pleuropneumonia.

The cases I am about to investigate are seventy-eight in number ; twenty-eight of them proved fatal ; and all were in a state of perfect health at the time when the first symptoms were developed.¹

¹ I have besides collected from 1821 to 1827, forty-five cases of pneumonitis, or of pleuropneumonia ; but these were relative to individuals, whose diseases occurred under different circumstances ; that is to say, they were persons already diseased, having

Of the fifty successful cases, three were bled on the first day of the disease, three on the second, six on the third, eleven on the fourth, six on the fifth, five on the sixth, six on the seventh, as many on the eighth, four on the ninth ; and the mean duration of the disease was, in the order pointed out, 12, 10, 20, 20, 22, 21, 19, 17 and 23 days.

But the relation between the length of the disease and the period of the first bleeding, will be made more evident by the following table :

1	2	3	4	5	6	7	8	9
10 3	7 3	19 3	19 3	28 2	13 1	24 2	19 2	35 1
12 2	10 2	29 3	12 2	17 3	16 2	12 4	12 1	11 2
14 2	12 2	20 2	15 2	40 2	23 3	19 2	18 1	17 2
		20	22 4	13 2	35 5	18 2	20 3	30 3
		16 3	12 4	21 2	17 2	15 2	13 2	
		17 4	21 2	13 2		27 2	21 2	
			25 3					
			28 4					
			40 2					
			16 2					
			12 4					
12 2½	10 2½	20 3	20 8	22 2	21 2½	19 2½	17 2	23 2

The figures upon the horizontal line above the columns indicate the day when the first bleeding was performed ; the figures on the left in each column mark the duration of the disease ; those on the right, the number of bleedings ; and those on the horizontal line below, show the mean duration of the disease and the average number of bleedings.

That is to say, if it were possible to establish

been affected, for a certain time, with pulmonary catarrh ; and I have decided to reject these facts from my analysis, in order that a just comparison may be instituted. No other fact has been excluded, so that I have in truth made a complete enumeration, or an analysis, of all the facts strictly analogous to each other, which I have collected.

a general proposition from so small a number of facts, it must be concluded that the antiphlogistic treatment, commenced the two first days of a pneumonitis, may very much abridge its duration; whilst after these two days it would make but little difference whether it was commenced a little sooner or a little later. But the amount of difference which exists between these two results, leads us to suspect their exactness; and a thorough examination does in truth show, that the influence of bleeding, when performed within the two first days of the disease, is less than it seems to be at first sight, and that in general its power is very limited.

Indeed — among the cases of the same column in which the antiphlogistic treatment was instituted on the same day, (those of the first and second excepted) the duration of the disease exhibits the greatest variety. Thus in the fourth column, some were convalescent on the twelfth day, others (not to take the extreme) the twenty-fifth and twenty-eighth. This we cannot attribute to the violence of the disease, which was the same; nor to the difference of the treatment, which was equally energetic and directed by the same physician. Whence it seems to result, rigorously, that the utility of bleeding has been very limited in the cases thus far analyzed.

Differences no less considerable in the length

of the disease would unquestionably have existed among the cases bled within the first twenty-four or forty-eight hours, if their number had been greater. And on the same supposition, the difference of the mean duration of pneumonitis, in subjects bled the two first days, and those who were bled at a later period, would have been less considerable. So that we should get nearer the truth, we should estimate the real difference effected in the progress of the disease by the greater or less promptness with which we have had recourse to bleeding, by taking the mean duration of the disease on the one side, in the cases bled during the four first days : and on the other, in those who were not bled until the fifth to the ninth inclusive. And then the mean duration of pneumonitis would be seventeen days among the first and twenty among the second.

But the average given by the table, is probably still a little too favorable in respect to the patients bled within the two first days, for another reason ; to wit, that, not having committed any error of regimen before the bleeding, these patients were in a condition the most favorable for treatment ; this was not the case with those, in whom bloodletting was employed at a later period, and among whom many in each group had committed errors in regimen ; some had taken strong drink, such as hot sweetened wine, one

or many days in succession, in a greater or less quantity ; some had even taken brandy. The length of the disease must certainly have been increased by these errors.

Age had no appreciable influence, every thing else being equal, upon the results stated : for this was nearly the same on an average among patients bled for the first time, before the fourth day, and among those who were not bled until after this period ; thirty-three years in the first set, and nearly thirty-six in the other. A fact however, which should not I think be advanced as a law, age having certainly a prejudicial influence on the termination of pneumonitis.

Nevertheless, in regard to the foregoing remarks on the causes which, independently of the period of the first bleeding, must have effected some difference in the mean duration of the disease, it will be said perhaps that the pneumonitis was less severe in the patients bled at a late period, than in those bled on the first days of the disease : that it was undoubtedly for this reason that the former delayed application for medical aid ; and that in this manner conditions, unfavorable to the rapid termination of the disease, were compensated. But having appreciated with all the exactness, of which I am capable, the symptoms experienced by patients at the commencement of their disease, and at their entrance

into the hospital, I have found cases of severe or mild peripneumonia in a nearly equal proportion among the different groups of subjects ; so that, supposing any mistake on my part, it could not be important enough to effect a material difference in the results stated, and to warrant us in rejecting the conclusions drawn from the analysis, which I have given. Physicians not much conversant with hospitals, or who seldom practise among the laboring classes, will not readily give credit to these remarks ; but those differently situated are aware that, whether it be from indifference, or dislike to hospitals, patients seldom enter until quite late ; even when their diseases have been very violent from the beginning.

Perhaps too it will be thought that I have not fixed the exact period of commencement and termination of pneumonitis with sufficient precision, and that its mean duration has been affected by this circumstance. But it seems to me, I have obviated any legitimate objections in this particular, by following in all cases the same method ; that is, I have regarded as the commencement of the disease, the period when the patient has experienced a febrile affection, more or less violent, which has been quickly followed or accompanied by pain on one side of the chest and by rusty sputa ; these two symptoms appearing at

the same time, or nearly the same time ; and I have regarded as the time of convalescence the period, at which the sick began to take some light nourishment ; three days at least after the febrile action had ceased ; although the local symptoms had not disappeared in every case ; that is to say, at a period when percussion of the chest did not always elicit a perfectly clear sound at the part affected, and when the respiration was not very pure ; the ear still discovering here and there some crackling and traces of crepitation. These are remnants of a severe morbid affection, which disappear in convalescence, and with a rapidity in proportion to the promptness of the antiphlogistic treatment.

Finally, the reader will ask, without doubt, whether bloodletting has been the only treatment, of any importance, which has been employed ; and in the cases, where other modes of treatment were employed, whether these other modes had not some influence on the mean duration of the disease ; or whether they had not counteracted in some degree the good effects of the bloodletting. To this I will answer that vesication was employed in a certain number of cases ; but vesication had no appreciable influence on the progress of the disease, as we shall presently see, in the following chapter : so that it will still appear that, in the cases, which we

have thus far examined, bloodletting has had but a very limited influence on the course of pneumonia.*

The facts relative to the fatal cases confirm these conclusions, and seem still further to limit the utility of bloodletting. Out of twenty-eight cases in question, eighteen were bled within the four first days of the disease, nine from the fifth to the ninth; and if on the one hand, we take together all the patients who were bled for the first time within the four first days of the pneumonia, whatever may have been its termination; and on the other hand all those who were bled at a later period: we have, in the order indicated, on one side, forty-one cases, of which eighteen, or about three sevenths were fatal; and on the other, thirty-six, of whom nine, or only one fourth were fatal. A startling and apparently absurd result; the explanation of which is found, to a certain extent, in the following table. This table, which relates to the fatal cases only, shows in each of the columns from left to right, the duration of the disease, the number of bleedings, and the ages of the patients; whilst the figure above each column indicates the day when the first bleeding was practised.

* From ten to fifteen ounces were taken at each bleeding.

	1	2	3	4	5	6	7	8	9
6	5 18	53 5 65	4 1 57	29 2 19	16 4 58	62 4 20	20 2 68	25 1 40	22 1 50
		12 3 69	16 2 54	29 4 46	8 2 63	10 2 40			
		8 2 65	6 3 30	12 1 85	9 4 24	29 3 24			
		12 1 55	6 4 47	15 3 37					
		17 7 75	47 2 75	17 1 67					
			11 4 45	20 3 22					
6	5 18	20 3 35 66	15 3 51	20 2 49	11 3 48	33 3 28	20 2 68	25 1 40	22 1 50

We see, in effect, that the patients who were bled within the four first days of the disease, with the exception of one in the first column, who was eighteen years of age, were older than those who were not actively treated until after this period, in the proportion of fifty-one to forty-three years : this difference may not seem great, but it may have had great influence on the issue of the malady. Indeed the difference in question, that of age, is much less, if, taking the fatal and successful cases indiscriminately, we add together on the one hand, all the patients bled within the four first days ; and on the other those who were not bled until a later period ; for we then find that the mean age of the first class is forty-one, and that of the second, thirty-eight. But it is nevertheless true, that the number of patients bled on the first day, who had passed the age of fifty, was nearly twice as great as that of the patients of the same age, who were bled at a later period. This must have had great influence on the mortality.

But it is not enough to have studied the effects of bloodletting upon the progress and termina-

tion of the disease ; its influence on each particular symptom must be separately investigated. Let us begin with pain.

Pain was not arrested by bloodletting in any of the cases bled within the four first days of the disease. On the contrary, it generally increased during the succeeding twelve or twenty-four hours : and its mean duration, usually in proportion to that of the disease, was six days among those who were bled during the four first days ; eight and a fraction among those bled at a later period. It yielded more readily to local than to general bleeding.

The *sputa* regarded as characteristic, were *adhesive, rusty, or like apricot jelly, and semi-transparent*: the mean duration of these sputa varied like that of the pain, or nearly so ; being five days in patients bled within the three first days, six in those bled within the three following, seven in cases where the bleeding was from the seventh to the ninth day inclusive.

The morbid character of the sputa became more distinct after bleeding, in the greater part of the cases, in which it was employed at the onset of the disease. On the contrary, the sputa were less morbid on the day following the bleeding among the patients who were not bled until a late period.

It seems to me, this can only be explained by

admitting that the disease had approximated its natural termination in this group, and that it was more or less distant from it, in the other. An important fact, which explains the difference of the effect of bleeding, in circumstances which are similar only in appearance, and which shows, with many others of the same kind, that we probably do not arrest inflammations at once, as is very generally believed.

As it regards *crepitation*, *resonance of voice*, *hægophony* and *dullness on percussion*, their ordinary length varied like that of the preceding symptoms ; that is, in the cases bled at a very early period, they were still more prominent, during one or more days after the first bleeding, than they had previously been ; whereas they diminished rapidly after the first bleeding when this was employed at a later period ; at least in the majority of cases.

The *acceleration of the pulse* continued four, five, six, seven days and more after the first bleeding, in the cases bled from the first to the sixth day of the disease. Sometimes it even increased from one day to another, between two bleedings. The effect of bleeding upon the pulse seemed more decided, when we practised it later than the period indicated. That is to say, in a considerable number of cases of this kind, the pulse became calm, three days after the vene-

section ; much more rarely not until four or five days. This undoubtedly depended, as was before remarked, with regard to the sputa, upon the circumstance that the bleeding was practised near the time when, in the natural course of the disease, the pulse was about to resume its natural state.

As was the case with the quickness of the pulse, the *heats and sweats* diminished rapidly after the letting of blood, only when it was done at a certain interval after the commencement. The sweats continued longer than the heat, and lasted proportionably longer than the other symptoms in those individuals, who were not bled for the first time until six days after the commencement of the disease.

Thus, the study of the general and local symptoms, the mortality and variations in the mean duration of the pneumonitis, according to the period at which bloodletting was instituted ; all establish narrow limits to the utility of this mode of treatment. Should we obtain more important results, if, as is practised in England, the first bleeding were carried to syncope ?

This practice deserves a trial, but great success cannot, I think, be anticipated ; since many cases, the history of which I have drawn up, and which were fatal, were bled to a sufficient ex-

tent. Among these there was one who was bled on the day of the attack, and who nevertheless died on the sixth; the vein having been opened five times, and the quantity of blood lost twelve, or sixteen ounces each bleeding.

ARTICLE SECOND.

Effect of bloodletting in erysipelas of the face.

Out of thirty-three subjects attacked with erysipelas of the face, and who were all in a state of perfect health at the time when they were attacked, twenty-one were bled. The mean duration of the disease was seven days and a quarter in those who were bled, and eight in the others. That is, after this time, the erysipelas did not extend, and the local symptoms, redness, hardness and thickening of the skin, diminished. It seems, then, that in the cases in question, bleeding shortened the duration of the disease three quarters of a day. For I do not take into account two other means of treatment which were used in nearly all the cases, whether bled or not. I mean purgatives and mustard foot baths.

Perhaps it will be thought that the difference between the two classes of cases would not have been so inconsiderable, had not the disease been

severe and extensive in those of one class, while it was mild, or slight and very limited in those of the other. But this has not been the fact ; and the erysipelas presented many degrees among those patients who were bled, as well as in those who were not ; so that we may consider them in this respect on an equality. Bloodletting was precluded in some cases ; either where the patients entered the hospital at a late period, or where the febrile action was so unimportant that derivatives alone were thought necessary. I will add that some of the patients were bled before they came under my observation, and it is not to be presumed that in all of these cases the febrile action was considerable.

As for the rest, I am about to enter upon details, which will give to the facts in question their real value in exhibiting them under another form.

The twenty-one patients bled were not all bled at the same period. One of them, a medical student, past thirty years of age, and of a strong constitution, was bled on the first day of the disease, and the erysipelas was not stationary, nor did it begin to subside until the eighth day from the commencement. The other patients were bled for the first time on the second, third, fourth, fifth and sixth days of the disease, and the mean duration in this class was seven, six, seven

and three quarters, seven and a half and seven and a quarter days. That is, it was nearly the same at whatever period they were bled. This would not be the case, were not the course of erysipelas of the face almost always uniform and very little affected by bloodletting. Otherwise, the effects of bleeding would have been very evident in the cases bled within the second or third day of the disease. Indeed it should be remarked that a majority of the cases, in which the local symptoms were the most violent, were bled as soon as the second, or third day of the disease, and at least twice. And, if we may not conclude that bloodletting was prejudicial in these cases, it must at least be acknowledged that its utility was not demonstrated.

It will be thought perhaps that if, instead of having had recourse to the lancet, we had applied leeches near the inflamed part, or even upon the part itself, we should have been more evidently successful. But the facts do not sustain this supposition. For in six patients, to whom, leeches were applied near the part affected on the second, third and fourth days of the disease, three of them besides were bled on the day after, and one on the very day of the attack : in these cases I say, the mean duration of the erysipelas was eight days and a quarter ; of course more considerable than in the others. I shall

not certainly attribute this to the leeches, but shall conclude, at least, that their influence on the progress of erysipelas was not such as had been supposed ; that it is even doubtful whether they have the slight degree of usefulness of general bleeding.

Objections may still be made to the inferences, which I think may be rigorously deduced, from the fact that patients attacked with erysipelas of the face are very often sensibly relieved, have much less redness of the face, during, or immediately after the bloodletting than before. This relief and paleness of the face, do indeed take place sometimes ; but these effects are momentary, and the progress of cure is not more rapid in these cases than in others. So that the only conclusion from this fact, is, that the immediate and the strictly therapeutic effects of remedies must not be confounded.

Again, as we have already seen in pneumonitis, we can readily explain how the utility of bloodletting in erysipelas of the face has been exaggerated, when we reflect on what has taken place in some instances, in which the evacuation was made at a late period of the disease. In fact, three patients who were bled on the sixth day of the disease only, showed a remarkable amendment on the following day in all the symptoms ; and this amendment progressed rapidly.

But does not every one see that this amendment is perhaps a mere coincidence only ; the erysipelas being near its most usual termination at the moment when bloodletting was practised ; and that the only reasonable presumption, in favor of bloodletting, is that it may have diminished the duration of the disease one half or three quarters of a day. Another proof, this, of the necessity of possessing an exact knowledge of the natural progress of diseases in order to arrive at a just estimate of the value of therapeutic agents.

Without an elaborate statement of the general symptoms subsequently to bloodletting, I will remark that in one third of the cases in question, the pulse lessened in frequency one day before the retrocession of the disease ; this having been at its state, as it is technically called, that is neither advancing nor receding. A fact, which is not unimportant, with reference to inflammation of the deep seated organs, the progress and decline of which is commonly estimated by the pulse ; since it indicates the necessity of waiting at least three or four days after the circulation returns to its natural state, before it can be satisfactorily ascertained that the inflammation has not left other, than trivial vestiges, in the diseased organ.

ARTICLE THIRD.

Effect of bloodletting in angina tonsillaris.

I have collected thirty-five cases of angina tonsillaris, occurring in individuals previously in perfect health. Twelve of these were slightly affected, the disease spontaneously disappearing, or nearly so, in four or five days; and I set these apart from my analysis, so that the remainder may be subjects of fair comparison among themselves. Having thus reduced the number of my observations, the proportion of violent and slight cases of angina was nearly equal among those, who were, and those, who were not bled. Inflammation of the tonsils existed in all the cases; and was, apparently at least, primitive, and complicated with inflammation either of the pharynx, or of the velum or vault of the palate; in the majority of cases, with the two last.

Out of the twenty-three cases in question, and in which the angina was more or less violent, thirteen were bled. The average length of the disease in these cases was nine days; in the others ten days and a quarter: and as the rest of the treatment, consisting of mustard foot baths, soothing gargles, and poultices to the neck, was the same with both classes of patients,

this difference, I think, can only be attributed to the employment, or omission of bloodletting.

The detailed examination of the facts confirms this proposition. Thus, the mean duration of the disease was eight days and a half in two cases, in which leeches were applied to the neck, at the beginning; the symptoms having diminished on the eighth day in one case, and on the ninth in the other. It was seven days and a half in two patients bled on the third day of the disease, which, nevertheless, was nearly as violent as in the two above mentioned. The duration was ten, nine, and ten and a half days in those who were bled on the fifth, sixth, and ninth days. This would not have been the case, had the bloodletting had much influence on the progress of the disease. It is also to be noted, that one of the cases, in which the disease lasted longest, ten days, is that of a patient to whom leeches were applied on the first and fourth days of the disease, the number of leeches, it is true, being small; but on the fifth and sixth days in large numbers, twenty-five each time; that in another in which the bleeding was done in the same manner and abundantly, fifteen ounces, on the third and sixth days of the disease, the symptoms did not diminish until the eleventh; and that it was very nearly the same in a third case, where twenty leeches were applied on the sixth

day of the disease, followed by a copious bleeding from the arm in the evening.

In the three last cases, the disease was without doubt violent, and it will be thought possible to explain the excess of its duration by its severity. I think the explanation excellent; but what is the conclusion, except that the influence of bloodletting on the progress of the disease is extremely limited?

From the same facts we should be led to question the great advantage of the application of leeches to the epigastrium in gastritis, or to any other part of the abdomen corresponding to the viscera presumed to be diseased. Indeed, what confidence can be placed on the *a priori* precepts commonly advanced on this subject, when the application of leeches the nearest possible to the affected organ, in erysipelas and angina tonsillaris, has so slight an effect that it is much less evident than that of general bleeding?

Further, let us particularly notice a remark important from its analogy with those before stated, namely: that in two cases, where bleeding was practised on the sixth and ninth days of the disease, the symptoms of the sore throat were much less on the next day and the day after; an amendment apparently owing to the bloodletting; but more readily, without doubt, and almost entirely, because the disease was near its

natural termination, at the moment when the vein was opened.

From the exposition of facts in this chapter, we infer that bloodletting has had very little influence on the progress of pneumonitis, of erysipelas of the face, and of angina tonsillaris, in the cases under my observation; that its influence has not been more evident in the cases bled copiously and repeatedly, than in those bled only once and to a small amount; that, we do not at once arrest inflammations, as is too often fondly imagined; that, in cases where it appears to be otherwise, it is undoubtedly owing, either to an error in diagnosis, or to the fact that the bloodletting was practised at an advanced period of the disease, when it had nearly run its course; that, it would be well, nevertheless, in inflammations of imminent hazard, pneumonitis, for instance, to try whether a first bleeding sufficient to produce syncope, from twenty-five to thirty ounces or more, would not be attended with greater success; and finally that, wherever I have been able to compare the effect of general, with that of local bleeding by leeches, the superiority of the former has appeared to me demonstrated.

I will add that bloodletting, notwithstanding its influence is limited, *should not be neglected in inflammations which are severe and are seated in an important organ*; both on account of its influence on the state of the diseased organ; and because in shortening the duration of the disease, it diminishes the chance of secondary lesions, which increase its danger; that, as it is not in our power to arrest inflammatory diseases at once, we must not endeavor to attain this imaginary end, by multiplied bleedings; for it must be remembered that a certain share of strength is necessary to the resolution of inflammation; since it is much more severe and hazardous in proportion to the feebleness of the patient, because this feebleness favors the development of secondary affections: finally, that these observations seem to show, that the use of the lancet is to be preferred to that of leeches in the diseases, which we have been considering.

CHAPTER SECOND.

New facts relative to the effect of bloodletting in acute diseases.

Since publishing the memoir, which forms the subject of the last chapter, I have observed at the hospital of la Pitié, a great number of cases of pneumonitis, of erysipelas of the face, and of angina tonsillaris ; and although, in the cases of pneumonitis I have employed bloodletting to the extent of twenty or twenty-five ounces and more, or even to syncope ; I have never seen these inflammations arrested in a single case. I believe even that the bloodletting, although usually carried to a greater extent than was practised in the hospital of la Charité, at the time when I made my observations there, has not been more decidedly successful. But these general propositions, founded upon facts trusted, for the most part, to the memory, have too little value to be much regarded ; and instead of discoursing, in a vague manner, upon the treatment of forty cases of erysipelas of the face, and one hundred and fifty cases of pneumonitis which have passed under my observation, the last four years, I shall confine myself to giving the reader an analysis of the facts, relative to these two dis-

eases, which I have collected with care during the time of my clinical lectures from 1830 to 1833.

ARTICLE FIRST.

Facts relative to the treatment of pneumonitis.

The observations under this head are twenty-nine in number ; in four of the cases the disease was fatal ; in twenty-five recovery took place, and the patients left the hospital perfectly well.

In all these cases, the patients were in excellent health, when the first symptoms of pneumonitis appeared.

Not a doubt can be entertained as to the character of the disease which affected them, all having expectorated rusty, viscid, semi-transparent sputa ; all having had, to a greater, or less extent, crepitous râle, bronchial respiration, and broncophony, with more or less dullness on percussion, in the corresponding part.

Of the twenty-five patients who recovered, no one was bled on the first day of the disease. The first bleeding was on the second, third, fourth, fifth, sixth and seventh days ; one case only excepted, that of a patient who was convalescent on the twenty-second day, and who was not bled until the fourteenth. And the disease lasted,

on an average, in the order pointed out above, fourteen, eighteen, fourteen, sixteen, nineteen, eighteen, and twenty-two days, according to the following table :

2			3			4			5			6			7			14		
15	2	28	11*	2	30	14*	2	32	9*	1	15	25*	1	20	11*	2	34	22*	1	16
16*	3	4	27	2	30	19*	2	24	28*	2	30	21*	1	20	19*	2	37			
11	3	50	28*	2	25	14*	2	27	11	1	20	12*	2	30	18*	2	38			
			9	1	18	12	2	35							24*	1	12			
						13	2	30							21*	2	30			
						15	2	24												
14	2½	27	18	1½	25½	14	2	29	16	1	21	19	1	23	18	1½	30	22	1	16

The ciphers on the first line indicate the days of the first bleeding ; those in each column, from left to right, the duration of the disease, the number of bleedings, the quantity of blood drawn. The figures, to which an asterisk is prefixed, also show that the patients to whom they refer, took antimony in large doses.

That is to say, that at the first glance, it would seem rather unimportant whether the patients affected with pneumonitis, were bled, for the first time, on the second, fourth, or fifth day of the disease ; since its mean duration was nearly the same, in the three lists of cases bled at these different periods. Nevertheless, adding together, on the one side, those who were bled for the first time, from the second to the fourth day inclusive : on the other, those bled afterward ; we find the mean duration of the disease to be fifteen days and a half among the first, and eighteen days and a quarter among the second. Hence it would seem fair to conclude that the influence of bloodletting, at a period more or less

near to the commencement of the disease, has been a little more marked in the cases now before us, than in those discussed in the first chapter ; in which the mean duration of the disease was seventeen days and a half and twenty days.

This difference, although slight, is worthy of remark, especially as it is found in each of the classes of cases, between those who were bled for the first time within the four first days of the disease, and between those who were bled later ; which seems to show that it is not accidental.

The difference in the results at the two hospitals is further remarkable, inasmuch as none of the cases, treated at la Pitié, were bled on the first day of the disease ; that these patients were, on that account, in a rather less favorable condition than those of la Charité, three of whom were bled on the first day. Might the difference be referred to this, that the first bleedings at la Pitié were a little more copious, than those employed at the same stages of disease at la Charité ? We shall hereafter revert to this circumstance which necessarily must have had some effect upon the duration of the disease.

Again, pneumonitis did not seem to me more commonly severe among the subjects bled before the fifth day of the disease, than among those who were not bled until after this period ; so that we could not attribute to the violence of the

disease, the slight effect of bloodletting employed on the first days.

But it will be said, perhaps, that bloodletting was not the only treatment employed, in the cases we are considering, and that other therapeutic agents may have interfered with its good effects. To this I reply, that antimony was indeed taken in large doses by many of the patients bled during the four first days of the disease, but that it was likewise administered to those, whose first bleeding was not performed until after this time ; both sides therefore being thus made equal, this circumstance may be disregarded as respects the present subject of consideration. But should any physician, strongly prejudiced in favor of bloodletting, presume that the duration of the disease would, generally, have been less, had not antimony been associated with bloodletting ; I would lead him to observe two cases in which this medicine was exhibited, where the disease lasted only eleven days ; while it was prolonged beyond this term in one of the patients, who did not take it, and who was bled on the second day. I should also request him to wait the further development of facts, which seem to me to show, that antimony, far from having been prejudicial, has been very useful to our patients.

Practitioners have been misled in believing it

possible to arrest pneumonitis, at its onset, by large bleedings, from having observed that in some cases, rare indeed, the bleeding is followed by a considerable amendment in the general and in some of the local symptoms, pain and dyspnœa. But the other phenomena remain, and are even augmented in intensity and extent after the first bloodletting, if this has been practised soon after the commencement of disease. And if then the patient is not accurately examined, the practitioner believes that the disease is arrested, when in fact there is only a diminution of the febrile action and some other symptoms. I have notes of a remarkable instance of this kind which occurred during the last year. I refer to the case of a young man, of pretty good constitution, who entered the hospital twenty-four hours after an attack of well marked pneumonitis. His symptoms were, extreme dyspnœa, much pain in the left side, hurried respiration, accelerated pulse, more than 110, increased heat of skin. He could not lie down in bed; the sputa were rusty, viscid, semi-transparent; the sound on percussion of the thorax, behind, inferiorly, rather dull; where at the same time there was heard a crepitous râle, and a confused respiration, approaching to bronchial; also, in some points, bronchophony without hægophony. He was bled from the arm to faintness, shortly after his entrance,

and lost twenty-five ounces of blood. Soon after he felt great relief, and on the following day, the diminution of the general symptoms was so evident, that many who were attending my visit, believed they had before them an example of pneumonitis arrested. The pain was less than on the day before, the pulse less than one hundred, anxiety had disappeared, and the expression of the face was natural. In the meanwhile the sputa retained their characteristic appearance, and the dullness of sound and bronchophony had become much more extensive than on the preceding day. This dullness of sound, the result of imperfect hepatization, could not be attributed to an effusion of liquid within the pleura ; for a fine crepitation, very near the ear, was audible on a part of the surface ; besides, the dullness of sound, which had extended toward the summit, had not on the whole increased ; so that the pneumonitis, far from having been arrested by an early and copious bleeding, had, since that, become more developed and extended : nor was its progress arrested until the fifth day, while convalescence¹ did not commence until the ninth or tenth. This has been observed quite frequently

¹ The convalescence of this patient, and of the others treated of in this chapter, is calculated from the data given in the first chapter.

among patients bled less copiously, at a later period of the disease ; and in whom the immediate relief from venesection was much less strongly marked.

Thus far then the results, which naturally flow from the study of the new facts before us, are in perfect accordance with those laid down in the preceding chapter.

Need I repeat that an excellent mode of arresting diseases is to confound them, or at least to make no distinction in the periods, at which such and such remedies were employed ; as I have pointed out in the preceding chapter.

Let us now study the progress and duration of each particular symptom ; and see if the correspondence in question extends to details.

Pain was not overcome in any case by blood-letting ; it was but little influenced by it ; for at the end of twenty-four hours, it was little less severe than on the day before, in the majority of cases. It was still present, on the sixth day of the disease, in a patient who was bled on the second day ; in this case fifty ounces of blood were abstracted, by two venesections, within forty-eight hours, and on the fourth day, five or six ounces more, by the application of twenty leeches, over the painful spot. Its mean duration was seven days and a half ; that is to say, nearly the same as in the preced-

ing cases ; whether we take those of pneumonitis of the lower or of the upper lobes ;¹ and in one of these last where the patient was bled to syncope, and lost thirty ounces on the third day of the affection, the pain on the following day was only a little diminished.

The *sputa* did not lose their pathognomonic character in a single case, the day after the first bleeding ; not even in the cases just stated, although the first bleeding was considerable. The duration of the morbid sputa was in proportion

¹ M. Andral was the first, I believe, who made the remark that pneumonitis of the upper lobe was more grave than that of the lower. It is true that pneumonitis of the upper lobe is more frequently met with in fatal cases ; but this is simply a coincidence ; for inflammation of the upper lobe seems more severe, merely because it occurs for the most part among the aged. Indeed, among the cases analyzed in this chapter, about one third had inflammation of the upper lobe, and were on an average fifty-four years of age ; whilst the mean age of those, who had inflammation of the lower lobe, was only thirty-five. On the other hand, pneumonitis of the inferior lobe was found in one of the fatal cases only ; and these facts which accord with all that I have observed for three years, scarcely permit a doubt of the correctness of my proposition.

Besides, as inflammation of the upper lobe is in some sort the pneumonitis of the aged, we should expect its progress to be different from that of the lower lobe, which occurs chiefly in the young. And in fact the duration of the pneumonitis of the upper lobe in patients who get well, exceeds that of the lower by three days on an average ; and this difference is very nearly the same for each particular symptom. This fact confirms what I stated in the preceding chapter respecting the presumed influence of age on the progress of the disease.

to the length of the disease ; so that on an average, they did not lose their specific character, until the seventh day of the affection in cases bled, for the first time, before the fifth day ; nor until the ninth, in those bled at a later period. And, the observation made in the first chapter is applicable here ; that the influence of bloodletting upon the sputa, was the more marked, when it was practised at the latest period ; so that in those bled at a late period, or after the fourth day, the sputa exhibited nothing remarkable after twenty-four, forty-eight, or seventy-two hours, from the first bloodletting ; while in those bled sooner, the specific character never entirely disappeared before three days, counting from the first bleeding. These differences admit of but one explanation, viz. that the disease, as I have said before, was drawing near its natural termination in the first cases, and that it was more or less removed from it in the latter.

Besides, if the duration of the disease was very variable in patients bled on the same day ; that of the specific character of the sputa was equally so ; for this continued, in different degrees, from four to eleven, or from seven to fourteen days, in patients bled for the first time, before or after the fifth day.

Crepitation lasted longer than the two symptoms already mentioned. It continued from ten

to fourteen days in cases bled before the fifth ; from ten to nineteen in those bled afterwards ; on an average twelve days in the first, fourteen in the second group. In no instance was it arrested.

The *respiratory murmur* was more or less completely altered during eighteen days on an average. That modification which is termed bronchial did not in any case yield to the first bleeding ; but was more affected by it, the later it was employed : in other words, if the first bleeding was instituted on the sixth day, the respiration would be less bronchial on the following day, than it was the day before ; while in patients bled on the second or third day of the disease, a similar change would not be effected until the third day after the bleeding.

Bronchophony, which depends upon the same causes as bronchial respiration, followed the same course, and had the same duration.

Dullness of sound on percussion occurred in all the cases, and lasted on an average until the nineteenth day ; gradually diminishing. And, with the exception of two patients bled on the fourth day of the disease, in whom the dullness of sound was much less the day after the bleeding than it had been the preceding day, an amendment in this respect began only from two to five days after the first bleeding ; and the ear-

lier the bleeding, the later, relatively, was the amendment.

In three patients, bled on the second day of the disease, the *pulse* fell on the following day from 120 and 100 beats per minute to 108, 80, and 96. But the next day, after a second bleeding, the beats were 104, 108, 90 per minute ; that is to say, it had diminished in frequency but very little after two bleedings.

It was the same in patients bled for the first time on the fourth day : in some of whom there was on the following day a temporary improvement in the pulse, in others none at all. But in a great majority of cases, where bleeding was not employed until after the fifth day of the disease, the pulse was less rapid on the following day ; and this amendment went on increasing subsequently.

In the cases now before us, as in those examined in the previous chapter, the influence of bloodletting upon the progress of the symptoms of pneumonitis has been unimportant, unless sufficient interval has elapsed between the attack and the venesection : and, no doubt, the reason is that, which I have before suggested ; that the disease had then more or less approximated its natural termination ; which termination was more distant, where bloodletting was employed at an earlier period. And these facts, as well

as those which relate to its duration, establish narrow limits to the utility of bloodletting in this disease.

Let us now examine the facts which relate to the fatal cases.

Out of twenty-nine cases referred to in this article, four, as we have seen above, were fatal ; i. e. one seventh. A proportion much less considerable than that given for the patients at la Charité, who were subjected to a somewhat different treatment, to which I shall presently advert.

Of these four patients, one only was bled a short time after the attack, the third and fourth days of the disease, and he died after one hundred and ten hours from the commencement of the disease.

The others were bled for the first time at a later period ; two on the fifth day of the pneumonitis, the last on the eighth. This last died on the nineteenth day, while the others on the eleventh and seventeenth. That is to say, the mortality was vastly greater among those bled for the first time after the fourth day, than with those bled before that time ; the fatality being one fourteenth for the former, and three fifteenths for the latter.

From the disproportion of mortality in the above cases, the conclusion is natural that blood-

letting soon after the attack, is much more important than one would have believed, from the statement of the cases of recovery. But this contradiction is in appearance only, and is removed when age is taken into the account.

Indeed, in the cases referred to in this chapter, there happened the opposite of what was noted respecting those referred to in the first; the mean age of the patients treated in this chapter, who were bled during the four first days of the disease, was much less than that of the individuals who were not bled until after that period; so that the former were aged thirty-nine years three months, the latter forty-seven years eight months. Besides, it is worthy of remark, that the age of the patient bled during the four first days, and whose disease terminated fatally, was forty-one, and those of three others bled at a later period, was sixty-one, seventy, and seventy-one.

An inspection of the following table will remove all doubt on this subject; the four fatal cases are placed below the others, and their ages may be readily compared with those above in each group. The number above each column indicates the day of the first bleeding. The number on the left of each column denotes the duration of the disease. That on the right the age of the patient, and the figures marked with an asterisk those who took antimony.

2		3		4		5		6		7		9		14	
15	36	11*	60	14*	45	9*	18	25*	61	11*	24			22*	58
16*	30	27	19	19*	23	28*	41	21*	58	19*	22				
11	29	28*	66	14*	50	11	25	12*	67	18*	18				
		9	20	12	24					24*	62				
				13	42					21*	60				
				15	61										
—		—		—		—		—		—		—		—	
		110 hours 41				11	71					19* 70			
						17	61								

It is evidently to the age much more than to delay in bloodletting, that we must attribute the great fatality of those cases which were not bled until four days after the commencement of the disease. So that the natural deduction from the facts examined with reference to the effect of bloodletting in pneumonitis, is in accordance with those which appear to me to have been drawn rigorously from facts previously collected at la Charité, and they all go to show that the effect of venesection on the progress of pneumonitis, is much less than is commonly thought.

But with respect to mortality, rather a remarkable difference exists between the subjects of the present analysis and those of the preceding chapter. To what shall this be attributed? Not to age; for the difference in this respect between the two classes was slight, and in favor of the patients of la Charité, whose mean age was forty, and that of the others forty-three: in which estimates I would be understood to include the fatal and successful cases. Nor can it be

presumed that the cases in which early bloodletting was employed, were more numerous in proportion at la Pitié than at la Charité. The tables relative to the sick of both hospitals forbid this supposition. Nor is it less impossible that it should be imputed to the different seasons at which the patients came under observation ; the majority of the patients of la Pitié having been admitted from January 1 to April 1, inclusively ; whilst the subjects of my observation at la Charité were brought there at different periods of the year. There remain then to account for the fact in question, the before mentioned difference in bloodletting, large doses of antimony, and the employment or omission of vesication. Let us examine.

If the patients of la Pitié were less frequently bled than the others, each of them was bled more copiously, especially at first ; and this difference, though not very great, must have had some influence upon the fortunate termination of the disease.

As to the effects of *antimony*, — this medicine was administered to sixteen of the patients, who recovered during a space of four to seven days, in increasing doses, from six to twelve grains in six ounces of the distilled water of the lime-tree flowers, sweetened with half an ounce of syrup of poppies ; and the patients took this in the

course of the day, in six or eight doses. The average length of the disease in these cases was eighteen days ; three days more than that of the patients who did not take the medicine ; so that it would appear at first sight, that the antimony, far from having promoted the cure, must have had a prejudicial influence.

This prejudicial agency, however, is only in appearance. The antimony was administered after frequent venesections, repeated on account of the augmented violence of the disease ; upon the eighth day, on an average ; and in cases where on an average, venesection had not been practised, until the fifth day : while in the cases where antimony was not administered, venesection was instituted on the third day. That is to say, antimony was not prescribed in any except severe cases, and under unfavorable circumstances ; this sufficiently explains, why the disease was protracted, in those to whom it was administered. Let us add another fact, and it is not necessary to insist on its importance, that the patients who took antimony, were generally older than those who did not ; the mean age of the latter being thirty-one, of the former forty-five. The difference is very great, and shows not only that antimony has not had the prejudicial influence, on the duration of pneumonitis, that we should at first sight have been tempted to ascribe

to it ; but that it must, in some cases, have accelerated its progress and prevented a fatal termination.

This last proposition seems to be further corroborated by the changes which almost immediately followed its administration. In fact, on the day after, fifteen out of the seventeen, who took antimony, were either a little, or very much better ; having evidently more strength, a better countenance, and less difficulty in respiration. Further, thirteen of them, in whom percussion was more or less completely dull over a certain extent, when the antimony was first given, showed a remarkable amendment in this respect on the following day ; the percussion being already more sonorous ; and these favorable symptoms constantly made new progress afterward.

The increase of strength, the day following the exhibition of antimony, is the more remarkable, as it induced frequent vomiting and purging. Out of seventeen cases, sixteen had copious dejections, eight to fifteen in number on the first day ; less frequently on the day following, and on the third or fourth these returned to their natural frequency. Vomiting was less frequent, and subsided sooner than the purging ; not lasting beyond the first day, and in five cases, or a little more than one third, was absent entirely.

Three of the patients who died, took anti-

mony, and were not improved the day after its exhibition. One of them only, had the evacuations above mentioned.

It appears then, that out of twenty patients, in a hazardous condition, to whom antimony was given, only three died ; this seems to me, to remove all doubt, as to the utility of large doses of antimony in the treatment of pneumonitis ; especially when it is considered that these three patients were sixty or seventy years of age.

The treatment of the patients at la Charité was further different from that at la Pitié, in this respect, that vesication was employed at the former, and not at the latter hospital. Can this circumstance have effected any difference in the unequal mortality of the two classes of patients ? Is it probable that vesication, employed at la Charité, exerted a happy effect on the progress of the disease, and would the patients at la Pitié have been cured more readily, and in greater numbers, had blisters been used in conjunction with antimony ? Let us look at the facts.

At the hospital of la Charité, blisters were not applied in all the cases which terminated favorably, but only where bleeding was so ineffectual, that fears were entertained for the result. Vesication was used in one half of the cases, or in twenty-five patients, whose first bloodletting was during the four first days of the disease, or later :

and the average length of the disease in these cases, was twenty-two days two hours ; while it was fifteen days eight hours only, in the other cases. An enormous difference, which would seem to show that the unfavorable conditions under which blisters were used, have not been sensibly affected by their action : and therefore we may infer that vesication was useless.

This was not the case, as we have seen, among the patients at la Pitié, to whom antimony was administered, and who were sick only three days longer than those who did not take it ; notwithstanding the very unfavorable circumstances, under which it was administered. For, besides the severity of the disease, which was very nearly equal among those who were blistered, and those who took antimony, the former had the advantage of age, thirty-five years and a half, on an average, the latter being forty-five years of age, minus a fraction. It would be difficult to attribute these differences in duration to chance, or to consider them accidental ; seeing that the length of the disease and the ages of the patients were nearly the same, among those treated by bloodletting alone, both at la Charité and la Pitié ; the mean age of the first being thirty-five, that of the second thirty-one ; the average length of the disease fifteen days and a third in the one set, fourteen days and one eighth in the other.

The following table will show more distinctly the difference just pointed out :

Mean age of the patients whose only treatment was bloodletting,	{	At la Charité, 35 years.
Mean duration of the disease among these patients,	{	At la Pitié, 31 "
	{	At la Charité, 15 days $\frac{1}{2}$
	{	At la Pitié, 14 " $\frac{1}{8}$
Mean age of patients, among whom bloodletting was not the only active treatment,	{	At la Charité, the patients to whom blisters were applied, 34 years $\frac{4}{5}$
Mean duration of the disease in these cases,	{	At la Pitié, the patients who took antimony, 45 years.
	{	At la Charité, 22 days.
	{	At la Pitié, 18 days.

And the next following table will enable the reader to verify the figures :

1	2	3	4	5	6	7	8	9
10 28	7 27	19* 27	19 66	28* 43	13 62	24* 40	19 30	35 64
12* 26	10 26	29* 23	12 20	17 34	16 60	12 26	12 33	11 20
14* 45	12 13	20* 24	15 22	40* 48	23* 19	19 53	18* 54	17 19
		20* 50	22* 23	13 50	35* 16	18 25	20* 19	30 23
		16* 20	12* 39	21* 59	17 36	15 27	13 40	
		17* 29	21* 50	13 29		27* 26	21* 44	
			25* 53					
			28* 54					
			40* 48					
			16* 22					
			12* 19					

The explanation of this table is like that given on page thirty-eight, excepting that the figures marked with an asterisk indicate the duration of the disease in those patients, to whom blisters were applied.

It should be further remarked that antimony and blisters were directed at the same period, the eighth day of the affection, on an average ; and that vesication was in no case followed by that speedy and decided amendment, which occurred in the other cases some hours after the use of antimony.

Since vesication has exercised no appreciable influence on the duration of pneumonitis among the patients at la Charité, it cannot be admitted that it would have shortened the disease among those, who were treated for the same, at la Pitié.

Further, I have not only rejected vesication from the treatment of pneumonitis, I have also ceased to employ it in pleurisy and pericarditis. I have treated, within five years, about one hundred and forty cases of pleurisy at la Pitié, (I include here only those who were in perfect health at the time they were attacked), without having had recourse to vesication in a single instance ; and they all recovered. I have had the same success in more than thirty cases of pericarditis occurring in individuals healthy up to the period of attack. It must be admitted that these facts render the utility of vesication in acute diseases of the chest still more problematical.

I was induced to reject vesication from the treatment of acute thoracic inflammations, because, as I have before said, an attentive study and rigorous analysis of facts forced me to acknowledge that acute inflammatory affections, far from preserving from inflammation, organs which are not the primitive seat of disease, are in truth an exciting cause of inflammation ; inso-much that the more severe the primitive inflammatory affection, and the more considerable the

accompanying fever, the more are the secondary inflammations to be dreaded. And how then can we believe that the effect of a blister is to check an inflammation, when this blister is one inflammation superadded to another? I acknowledge that this method of reasoning was not rigorous; it was mere reasoning by analogy: but it was an analogy drawn not from animals to man, not from the man in health to the man in disease; but from the sick to the sick himself; almost a certainty. I might in this view of the subject, without making myself liable to reasonable censure, be justified in trying to dispense with vesication in acute inflammations of the chest; and with these facts before us, all must allow that much has been taken for granted in regard to vesication under the conditions in question;—and that in all cases its action should be rigorously examined.

Shall we say then that blisters must be abandoned in every case? Assuredly not. I will not even say that it has been rigorously demonstrated that they are useless in every inflammation; I speak only of thoracic inflammations, in which their usefulness is neither strictly demonstrated, nor even probable. But one thing is most assuredly beyond question, and we should never be weary of repeating it; that the therapeutic value of blisters is not known; that it

must be studied by the aid of numerous and carefully noted facts, just as if nothing at all were known about it.

One other therapeutic agent demands the reader's attention: I mean the *syrup of poppies*, (diacodium¹), which was administered to those patients who took antimony; and which did not retard the amendment above noted, as having quickly followed the exhibition of that medicine. If the syrup was not injurious in this case, should this be attributed to its association with antimony? I doubt it; for we have little else than reasoning, with regard to the action of opiates in the treatment of inflammation; and I have collected some facts which show the great amount of our prejudices concerning the effect of opium. Opium, it has been said, should be avoided in the treatment of diseases attended with cerebral symptoms, because it acts upon the brain only by means of an engorgement of the cerebral vessels, and that, in this way, we should rather increase, than diminish the evil. But who has proved this mechanism? No one: for no one has proved that cerebral symptoms always depend upon engorgement of the vascular system of the brain. Resting on these two considera-

¹ An ounce of this syrup is very nearly equivalent to a grain of opium.

tions, that, on the one hand, it is not proved that cerebral symptoms, those, for instance, which are attended with agitation of the limbs, depend upon engorgement of the cerebral vessels ; and on the other, that, the mode of action of opium is not known : during the past year, I have administered this medicine to two young women affected with chorea ; in these cases the relief was immediate, and in a space of two weeks, the disease was happily terminated. Four times too, within four years, I have administered the syrup of poppies to patients under the typhoid affection, who had had subsultus tendinum, for twenty-four or forty-eight hours. I began with a dose of three drachms, rapidly increased to an ounce in the course of the day ; on the following day the subsultus had lessened, and it never recurred, in the same degree, as before the administration of the opiate.

Be this as it may, the following are the results of the investigations, in this and in the preceding chapter :

1st. That bloodletting has a happy effect on the progress of pneumonitis ; that it shortens its duration ; that this effect, however, is much less than has been commonly believed : but that patients, bled during the four first days, recover, other things being equal, four or five days sooner than those bled at a later period.

2d. That pneumonitis is never arrested at once by bloodletting, at least, not on the first days of the disease. If an opposite opinion is maintained, it is because this disease has been confounded with another ; or because, in some rare cases, the general symptoms rapidly diminish after the first bloodletting. But then the local symptoms, crepitation, &c., for the most part, continue to be developed not the less for this evacuation.

3d. That age exerts great influence on the rapidity of the progress, and on the favorable or unfavorable issue of pneumonitis.

4th. That where bloodletting proves ineffectual, consequently, in severe cases, antimony, in large doses, acts favorably, and appears to diminish the mortality.

5th. That vesication has no evident influence upon the progress of pneumonitis ; and that it may be dispensed with, in the treatment of pleurisy and pericarditis, occurring in healthy subjects.

But, notwithstanding the good effects of large doses of antimony, in the treatment of pneumonitis at la Pitié, it will be said, perhaps, that the mortality at this hospital was much greater than that announced by many respectable physicians, under the same circumstances ; particularly by the illustrious Laennec.

Indeed, in his work on diseases of the chest, we meet with the following statement :¹ “ In the year 1824, at the clinique of the Faculty, I treated with antimony, twenty-eight cases of pneumonitis, either pure, or complicated with a slight pleuritic effusion. All these recovered, with the exception of one cachectic old man, whose mind was already much impaired by age, and who took but little antimony, because he could not bear it. These cases, nevertheless were, for the most part, very severe. In the course of the present year, &c. &c. the mortality was a little less than one in twenty-eight.”

A little further, p. 504, he says, “ The results I have just stated are more favorable than those lately published by M. Rasori ; this may depend upon two causes : first, that peripneumony can be detected much sooner by *auscultation* than by the ordinary symptoms ; and secondly, that M. Rasori has apparently included many cases of simple pleurisy, or pleuropneumony with predominance of pleurisy, under the head of pneumonitis ; for it is impossible to distinguish these cases without the aid of auscultation ; and we have before remarked that, in the treatment of pleurisy by antimony, we ought not to expect the same success as in pneumonitis.”

¹ Second edition, 1st volume, page 500.

One remark, with regard to these statements of Laennec, will no doubt have occurred to the reader ; that they are deficient in details concerning the number of bleedings, the age of the patients, the period at which they were bled, the period when they took antimony, and the mean duration of the disease ; so that in consequence of this deficiency, we are not able to institute a comparison between these observations and others.

Another much more important remark is that, in a certain number of cases, Laennec trusted entirely to auscultation, in making his diagnosis ; that he considered crepitation, independently of every other local symptom, to be an infallible guide : so that he must have admitted many as cases of pneumonitis, in whom there existed crepitation only, without rusty, semi-transparent sputa ; without a more or less complete alteration of the respiratory sound ; and without any degree of dullness, on percussion, at the part affected.

We all know how highly cultivated were the senses of Laennec ; how delicate his ear. Nevertheless, as there is not a great difference between a crepitant râle, rather coarse, (for it is not always equally fine,) and a subcrepitant, rather fine, Laennec may possibly have been deceived, and taken one of these râles for the other, in a considerable number of cases.

In this case, he must have confounded acute pulmonary catarrh with pneumonitis, as the former affects the last ramifications of the bronchiæ, and is accompanied with a subcrepitant râle ; and it was without doubt, in consequence of such mistakes that there is so wide and important a difference between his results and my own : for acute pulmonary catarrh is not fatal, when it occurs in healthy subjects, whatever be the treatment ; those cases perhaps excepted, in which the whole of both lungs is affected.

Some other physicians, since his time, have undoubtedly fallen into the same error ; for we cannot otherwise explain how talented and honorable men, of unquestionable honesty in scientific researches, should have been more successful than even Laennec, in the treatment of pneumonitis with preparations of antimony.

In the same way, we can account for the fact, that double pneumonitis, terminating successfully, is so frequently met with, by some men, and so seldom by others. Indeed, out of all the cases investigated in this chapter, there was but one who recovered, among those affected with double pneumonitis. Further, the inflammation of the lung, secondarily affected, was but trifling ; it was less in extent than the palm of the hand.

In cases of pulmonary catarrh, affecting the extreme bronchiæ, it is indeed extremely rare,

not to find a subcrepitant râle at the lower part of the back on both sides. It cannot, therefore, be too often repeated, that auscultation, like all the most accurate modes of investigation, can lead to true conclusions, only when its results are compared with those obtained by other methods. Laennec himself has given cautions of this kind, which however he has not always observed.

ARTICLE SECOND.

Effect of bloodletting in erysipelas of the face.

During the time before mentioned, I collected eleven cases of erysipelas of the face, occurring in individuals perfectly healthy up to the time of attack; and they all recovered, according to my constant experience in similar cases. Six of them were not bled and did not take any purgative: the average duration of the disease was eight days and a quarter. It was ten and a half in those who were bled.¹ This difference may readily be explained, by the degree of severity of the disease, which was greater in these last than in the others.

¹ It is scarcely necessary to inform the reader that I have fixed the duration of the disease, in these cases, according to the data, stated in the preceding chapter.

Four of the patients were bled once only, and that on the third or fourth day. Among these, the disease lasted six and a half days, in one case, ten and twelve in two others, and sixteen in the fourth : this last was a patient, who was bled to twenty ounces on the third day. After this, how can it be believed that erysipelas of the face may be at once cut short ? The fifth and last patient lost sixteen ounces in two bleedings, on the third and sixth day of the disease, which disappeared on the eighth.

These, and similar investigations, in the first chapter, lead to the same results, and establish narrow limits to the utility of bloodletting in erysipelas of the face.

I have collected, in my clinical course, four cases only of angina tonsillaris ; and these of course are insufficient for analysis ; I therefore pass on to the third chapter.

CHAPTER THIRD.

Examination of the method, followed in the preceding chapters, to determine the therapeutic effects of bloodletting and antimony.

In studying the history of medical agents, it is not most important to ascertain their immediate action on the animal economy, but their therapeutic action, properly so called; in other words, their influence upon the progress and termination of different diseases. It has therefore been my constant object, in the two preceding chapters, to obtain such information concerning bloodletting and antimony; and to this end, I have followed a method which seems to me to be at once natural and rigorous. Indeed what was to be done in order to know whether bloodletting had any favorable influence on pneumonitis, and the extent of that influence? Evidently to ascertain whether, other things being equal, the patients who were bled on the first, second, third or fourth day, recovered more readily or in greater numbers, than those bled at a later period. In the same manner it was necessary to estimate the influence of age, or any other circumstance, on the appreciable effects of bloodletting. In other words, whether patients, in such circum-

stances, recovered sooner, other things being equal, under the influence of bloodletting, than others in different circumstances. And as to the details of the plan pursued in this inquiry, there was one course which seemed almost necessary. It was requisite to form one class of such patients as were similarly situated, another of those in somewhat different circumstances, to take the mean duration of the disease in each class, to compare and to draw conclusions.

This method, however, the simple exposition of which, ought to be a sufficient demonstration of its necessity, this method has been criticised by more than one physician. Let us see if there is any foundation for the attacks upon it ; and if it be possible to arrive at rigorous results, at any thing like a demonstration in therapeutics, without having recourse to it.

The first, and apparently the most weighty objection to the method in question, is that it is difficult to collect a sufficient number of cases of any one disease, which shall be identical ; especially if it be remembered that two cases of disease will hardly be found alike in every particular.

Without doubt, if, in order that two cases of the same disease may be classed together, it is considered essential that the individuals should be perfectly equal in age, and mathematically

similar in strength, stature and flesh ; if the disease must be exactly at the same stage, of the precise extent, (supposing it possible to measure it) : if the febrile action must be similar to such a degree, that the pulsations of the arteries in the two cases must not vary two or three strokes ; if such are the conditions of the required resemblance, it is impossible that they should ever be found united ; any more than that two leaves on the same tree should be found exactly alike in form, color and thickness. And as there is an evident necessity of uniting similar facts, in order to classify them and draw from them correct conclusions, it would follow that there would be nothing but individualities in medical science ; that it would always be impossible to attain any general principle whatever, even in pathology ; and also that there would be no means of describing the leaf of a tree in general terms. Experience, fortunately, enables us to appreciate the value of such conclusions, and of the assertion which gives rise to them. A leaf of a tree once well described may always be recognised ; and general principles of pathology, once clearly defined, can always be verified under circumstances similar to those, in which the subjects were placed, from whom such general principles were derived. Thus, in truth, we can form a class of facts bearing sufficient resemblance, one to

another, and from hence deduce laws which every day's experience verifies.

Reasoning *a priori*, as is the habit of those physicians who oppose this method which is known as the numerical method, we might, we necessarily should infer from the diversity in temperaments, in height, intelligence and many other circumstances easily estimated, we should infer that differences not less evident, existed in regard to the deep-seated organs and their functions ; and maintain, with respect to the stomach, for example, that as many kinds of food were necessary as there were individuals. Experience however shows that, in spite of these striking and indisputable differences between persons most resembling one another, nine hundred and ninety-nine out of one thousand, who differ in age, sex, temperament, &c. live on the same food, prepared in the same manner.

Experience also shows, and it is to experience we must appeal on every disputed point, that certain medicines, administered in the same disease, to individuals of different age, strength, temperament, &c. are almost uniformly successful. For instance, drastic purgatives in painter's colic, cinchona in intermittent fever, &c. Whence it follows, on the one hand, that a perfect, ideal resemblance is not necessary to the classification of facts ; and on the other, that a

truly efficacious medicine will exercise its influence in spite of differences in those, to whom it is administered ; the malady itself seeming to efface such differences.

With regard to cinchona, it will perhaps be said that the argument is not conclusive ; this medicine having been administered in intermittent diseases. But of what importance is that consideration ; so far as regards the subject under discussion ; since the patients, to whom the cinchona was given, offered all the varieties in respect to age, sex, temperament, strength, the period of the disease, &c. &c.

If there are many circumstances, which it is not necessary to regard, in making a general estimate of the effects of the therapeutic agents employed in the treatment of painters' colic and in that of intermittent fevers ; one may also disregard them in the treatment of many other diseases. In any epidemic, for instance, let us suppose five hundred of the sick, taken indiscriminately, to be subjected to one kind of treatment, and five hundred others, taken in the same manner, to be treated in a different mode ; if the mortality is greater among the first, than among the second, must we not conclude that the treatment was less appropriate, or less efficacious in the first class, than in the second ? It is unavoidable ; for among so large a collection, simi-

larities of condition will necessarily be met with, and all things being equal, except the treatment, the conclusion will be rigorous. In this manner has the treatment of Asiatic Cholera been estimated ; and none, unless those whose reputations have been concerned, have thought the method a bad one. Indeed I should like to know how we should proceed to satisfy ourselves on this point without counting.

Let us further remark that the objection made to the numerical method, to wit, the difficulty or impossibility of forming classes of similar facts, is alike applicable to all the methods that might be substituted ; *that it is impossible to appreciate each case with mathematical exactness, and it is precisely on this account that enumeration becomes necessary ; by so doing, the errors, (which are inevitable,) being the same in two groups of patients subjected to different treatment, mutually compensate each other, and they may be disregarded without sensibly affecting the exactness of the results.*

One of the objections made to the numerical method is that similar facts cannot be brought into comparison, because there is great difficulty in determining the commencement of a disease, and an impossibility of estimating its degree, or violence by its duration. This objection might be fully answered by referring to preceding remarks on this head. But it is perhaps better to

reply directly and briefly. Without doubt it is difficult to fix the period of the commencement of a disease, and no one perhaps has insisted on this circumstance more strongly than myself. At the same time it is not impossible to decide this point, either in acute or chronic diseases ; if we except some few patients who are deficient in intelligence or in the power of recollection ; and the cases of all such should be set aside and regarded, in many respects, at least, as of no value. And it is also perfectly true, that it is impossible to measure the degree or violence of a disease by its duration. But who has said that these two things were the same, and always proportioned to one another ? In judging of the degree of a disease, can we not rely on the violence of the febrile action, the pain, the prostration of strength ? and on certain symptoms peculiar to each affection ? In pneumonitis, for instance, on the dyspnœa, or the results of auscultation and of percussion, &c. &c. ?

I will add, that it is still more difficult to fix exactly the period of termination than of the access of a disease ; this however, must be done, whatever method is adopted to appreciate therapeutic agents : it must be done at any rate, even if, without any method at all, one should be confined to a vague and uncertain interpretation, for rigorous it cannot be, of isolated facts.

Upon the subject of bloodletting in particular, it has been already said that its influence, in an absolute sense, cannot be estimated any more than that of other therapeutic agents. For instance, the patients may be bled in the middle, or at the termination of a pneumonitis ; this disease may be mild, or severe ; the abstraction of blood copious, or moderate ; you cannot therefore, it is added, form a judgment of its effect, whether beneficial or injurious ; unless you have described precisely the *motives* for having recourse to it, and have pointed out exactly the signs, which have indicated its employment.

If by motives it is understood that no therapeutic agent can be employed with hope of success, unless the case can be recognised as analogous to others, in which it has been used with advantage ; I comprehend the proposition, and view it in the same light ; it is simply the application of experience to therapeutics. But if by motives, as by indications, are meant merely *a priori* considerations : this view is altogether hypothetical ; it is a kind of experiment, founded on what are called rational views, which should be resorted to only for want of a better guide in case experience has not instructed us ; and I repel it with all my strength.

The foundation, upon which I think it possible to establish the value of therapeutic agents, has

appeared so insecure to many others, that they are astonished at the excess of my confidence ; it has been thought that I should have escaped from error if I had first of all sought to make myself master of the spirit of mathematical science. What is the calculus, it has been said. A method, which strikes off all differences in the objects to which it is applied, in order to transform those objects into abstract and absolute quantities.¹

¹ This and the preceding objections have been brought forward anew by a physician, whose name I shall not quote for fear of the imputation of revenge, and this I am far from entertaining. This physician has said "By invoking the inflexibility of arithmetic, in order to escape the encroachments of the imagination, one commits an outrage upon good sense; as if it were possible to heap together flowers, houses and birds, and to derive from the medley, fish and fruits!" In other words, to subjoin to one case of pneumonitis, another of the same affection, apparently as severe, occurring in individuals apparently under similar circumstances, but which may differ a little in fact; this is the same thing as the bringing together of flowers and houses! What sort of readers does the author think he is addressing?

Following this objection are two others, which are analogous, and refute themselves.

That the numerical method should have opponents is natural, and was easily foreseen; for what proposition is there, except an axiom, which is adopted unanimously? Fortunately for the progress of science, the numerical method is considered by the most judicious and experienced men, as a necessary instrument for establishing general principles in medicine; and attacks on it will be futile; for the only auxiliary will be the repugnance unfortunately so natural, to great and continued labor: and it will be sufficient to ensure the progress of science that this repug-

To this I reply that the calculus, as I employ it, does not efface differences : *it supposes them* ; it limits itself to combining similar unities in order to compare them with parallel unities, these being subjected to somewhat different influences ; that if, after all, as has been before remarked, it should sometimes be necessary that facts should be combined, which are not strictly similar ; the error will be distributed through the different groups or classes of facts, and will be equalized ; so that a comparison can be instituted between several groups without altering the result.

In fine, it is by the results that the value of methods is to be appreciated ; men have for ages devoted themselves to therapeutics, and the science is still in its infancy. Some course must be pursued different from that, which has been hitherto. Able men have never been wanting to science ; and it is to the method pursued, or rather to the want of method, that we must attribute the actual state of therapeutics. Let us bestow upon observation the care and time which it demands ; let the facts be rigorously analyzed in order to a just appreciation of them ; and it

nance be overcome by some individuals of industrious habits. I will add, that the necessity of the numerical method can be completely demonstrated only by the objections of its opponents ; it will in truth be established by their labors.

is impossible to attain this without classifying and counting them ; and then therapeutics will advance not less steadily than other branches of science.

Hitherto there has been so much fluctuation in medicine ; results, (as they have been called) so variable, so often belied by the facts ; so seldom has experience verified what is found in books, that it will perhaps be said that the science, which by my figures I render so certain, this science will desert the practitioner by the bedside. Without doubt science will desert the physician, who makes an improper application of it ; but how could it abandon him, if he employs it with discernment ; science, *true science*, I mean, being but a summary of particular facts. In proof of the truth of these propositions I would remind the reader of the results, which I arrived at by the numerical method, six years ago, with regard to the effect of bloodletting in acute diseases ; and that these results have been since confirmed by the analysis of new facts collected at the Hospital of la Pitié. I will add that an industrious young physician, M. Bachelier, published in 1832, in his inaugural dissertation, a series of facts, which confirm all that I have said and observed on the subject of bloodletting ; and as this cannot be considered an accidental coincidence, it decides

incontrovertibly in favor of the method, which leads to such results.

The objections made to the numerical method, as applied to therapeutics, are then unfounded. Therapeutics cannot advance without it. To say that this method is not necessary, in order to study this subject with certain advantage, is to deny the necessity of grouping facts according to their resemblances, and then of numbering them in order to give an account of the action of therapeutic agents : for in truth, counting has no other object. Besides, such a denial betrays excessive prejudice and a forgetfulness of what is done in common affairs. Indeed when physicians are called to attend on a patient, if after having agreed upon the character and kind of disease, one of them differs from his colleagues in regard to the treatment proposed, what does he do to sustain his views ? He does not trust (I refer to practitioners of experience) to theoretical arguments, to *a priori* considerations, for these would never produce conviction in any one ; but, he urges the preference of his own plan, on the ground that he has seen it more often successful than any other proposed. That is to say, — he reasons as if he had counted ; although without having done so, I agree ; and this reasoning is a tacit avowal, or proof that one cannot determine the operation of a therapeutic agent without in-

quiring whether, if administered under stated conditions, and such as are apparently similar, it has not been more often successful than any other.

It will be said, perhaps, that the method in question, although it may enable us to show, generally, that this or that medicine is better than another, does not show why a certain individual, affected with pneumonitis for example, and treated just like his neighbor, who is apparently in the same condition: it does not show, it is said, why the former recovers more slowly than the latter. To this I reply, that the advantage derived from the numerical method is very great, and cannot be obtained from any other; but that, when certain patients, who were thought to be under similar circumstances, and who are treated in the same manner, recover health after very unequal periods of disease, this must be attributed to a want of such an exact resemblance in the cases, as they had been thought to possess; and this is only another reason for studying the sick with great care, the more exactly to ascertain the points of similarity and dissimilarity among them. But in order to know whether the dissimilarities are as important, as we are inclined to consider them, whether they have in fact a marked influence upon the action of therapeutic agents, whether they explain the difference in

duration of diseases ; it is obviously necessary to arrange on one side, all the cases in which dissimilarities, not noticed at first view, were found to exist : on the other, the cases in which they do not exist : to count both lists : and if the duration of disease in each patient on the same list presents differences less than those in question, add these durations, take the average, and compare it with the other list. In other words, we must still count. It is clear that until similar facts have been thus brought together, counted, &c. there is scarcely a probability in favor of this or that opinion.

Yes, I do not hesitate to say it, and the attentive reader will unite with me in the conviction : between him who counts his facts, grouped according to their resemblance, in order to learn what value he can attach to therapeutic agents, and him who does not count, but who contents himself with repeating *more or less, rarely or frequently* ; there is the difference of truth and error ; of a thing clear and truly scientific on one hand, and of something vague and worthless on the other : for what place can be assigned in science to that which is uncertain ?

No one denies the necessity of a large body of facts, in order to decide upon the best treatment of any disease whatsoever ; *but of what service are these facts if they are not enumerated ?*

Still further objections are raised : it is urged in opposition to the numerical method, that the amount of facts, upon which it operates, is always limited ; and that to be as valuable in all respects, as it is thought to be by its friends, it should be employed upon a much larger body of cases than a single observer can collect. But this very objection is one of the strongest arguments in its favor ; since, as each practitioner counts, limited numbers added to limited numbers, will at last produce such considerable results, as not only to determine the law, but to present it in a form of arithmetical exactness.

We are constantly told of the experience of ages in medicine ; but how can this experience ever be embodied, if those who write, instead of saying I have seen so many and so many times, merely say I have often seen, or seldom seen ? By determinate observations the experience of one man can be added to that of another man. But how can the experience of one who says more, less, rarely or frequently, be added to that of another, who in like manner says, more or less rarely, more or less frequently ? Suppose thousands of authors to have proceeded in this manner, it is as if there had been but one : and in many respects, as if there had been none at all. *If then there is a means of embodying the experience of ages, it is the numerical method.*

This proposition will undoubtedly soon be the current opinion ; and then we shall hear no more of medical tact, of a kind of divining power of physicians. No treatise whatsoever will continue to be the sole development of an idea, or a romance ; but an analysis of a more or less extensive series of exact, detailed facts ; to the end that answers may be furnished to all possible questions : and then, and not till then, can therapeutics become a science.

Let us finish what remains of this examination by a rapid glance at some monographs on the subject of bloodletting. A few quotations will be sufficient to show the course that has been pursued ; and will inform us whether any one of the numerous points discussed, has been decisively settled by them ; and whether the method I have laid down is not the only one, which would have decided the questions in debate. The works, to which I would for a moment direct the reader's attention, are those of Quesnay, Fauchier, Fréteau, Vieusseux, and M. Polinière.

*Quesnay*¹ begins by remarking that experience

¹ *Traité des effets et de l'usage de la saignée*, une vol. in 12mo. 1770.

shows in the main the utility of bloodletting in many diseases; but that experience is so equivocal as to the success of this remedy, that practitioners think differently in different cases; that all nevertheless appeal to experience, in support of their different opinions, and of the theories which they have framed to account for the effects of bloodletting, &c. &c. (page 2).

These remarks, which were true in the days of Quesnay, are unfortunately so at the present day. But instead of searching for the deficiencies of experience, if one would not dignify with the title of experience something, which does not in the least resemble it, or which is nothing but its shadow; the author concludes merely by saying, "that experience which conducts us in the dark regions of practice, is a faithless guide (page 3 and 4); that if our knowledge on the subject of bloodletting is indefinite, and uncertain, it is because the ideas we have entertained in respect to its general and primary effects, have been very vague and obscure." (p. 5).

With these views, he consequently endeavors to show, by a multitude of arguments, which have no other foundation than some facts noted in a physical order, that the primitive effects of bloodletting, upon which all its influence depends, may be reduced to three; evacuation, spoliation, and *dimotion*: from hence numerous

indications follow, still less to be depended upon, than the blind experience of practitioners, which Quesnay treats with so much contempt. There is not a shadow of direct demonstration; we might suppose that he would have thought himself disgraced by attempting it. And we are not astonished that after having denied revulsion and derivation, in his view of the general effects of bloodletting, without deigning to refer to facts; we are not astonished at the sort of fatuity with which he cries, "The discovery of the circulation of the blood has dissipated the chimeras, which led the great masters into error. A more rigorous examination of the laws of the circulation will finally scatter the prejudices, which have hitherto been entertained, respecting derivative and revulsive bleeding."

This is not the place to inquire whether the revulsive or derivative effects, attributed to bleeding, are real or imaginary; but all will agree, that an appeal to the laws of the circulation for a decision on this point, is a proceeding entirely opposite to that required in the sciences, where theory, or general principles, must of necessity be deduced from particular facts. Unfortunately, Quesnay has no other method, and certainly the incomplete experience of practitioners cannot take rank below his assertions.

Besides we find that the doctrines of revulsion and of derivation have not been neglected by any of the authors above mentioned ; all have touched upon them ; Polinière and Fauchier deny them ; Fréteau and Vieusseux admit them.

Amidst this conflict of opposite opinions, how shall the question be satisfactorily settled ? Not in the least degree by following the example of the above named authors : not by denying the doctrine *a priori*, as Quesnay and Fauchier have done ; nor by citing some cases in support of them, after the manner of their antagonists ; since one may cite on the other side such as would favor an opposite doctrine. But by collecting the greatest possible number of facts, taken indiscriminately, provided they are exact ; these facts being all of them relative to patients under the same affection ; some of whom shall have been bled, the nearest possible to the seat of the disease ; others, at the point most distant from it ; by analyzing all the facts ; by carefully estimating the age, sex, and strength of the individuals ; and then seeing if, in a certain number bled near the part affected, the disease has made more favorable and rapid progress ; or, on the other hand, has more often terminated fatally, than in another group bled at a point the most distant from the seat of disease. Having once completed the analysis, the question will be de-

cided, provided the facts are sufficiently numerous. Indeed, how can the problem be clearly solved in any other way?

Fauchier,¹ whose work on the indications of bleeding was crowned by the Medical Society at Tubingen, in 1807; Fauchier, after having stated the main points which he proposes to discuss, remarks that they all belong to clinical medicine; that they should all for this reason be decided by experience alone. (page 12). And, some pages beyond, forgetting this profession of faith, he denies the doctrines of derivation and revulsion, because he believes them not to be in accordance with the laws of the circulation. (page 21). That is to say, he follows the course of Quesnay, who, at least, justly appreciated, what the physicians of his time called experience; whilst Fauchier, thinking that experience sufficient, contents himself with being its echo; giving, almost exclusively, as precepts, the most commonly received practice of his time; for his work is in truth nothing else. Yet, this work, (and let us not forget it, for it shows the spirit of the time,) this work was crowned by a Medical Society. Moreover, as if to remove all kind of doubt in regard to his views of experience, as

¹ *Nouvelles indications de la saignée*, 1 vol. 8vo.

applied to therapeutics, Fauchier endeavors, after the example of Quesnay, to determine the general effects of bleeding ; and he concludes from his researches, that the cases in which we must employ bloodletting, are these : first, where there is plethora ; second, where there is too great frequency and excess of strength in the contractions of the heart ; third, where there is a depraved tension of the solids ; fourth, where there is excess of strength ; fifth, where there is increased heat. (page 70).

These principles being laid down, the author deduces from them, without difficulty, the cases in which bleeding should be practised ; this is an error at the outset, as I have already remarked with regard to Quesnay. For in sciences of observation, general principles can be the result only of particular facts, sufficiently and properly estimated ; so that, in order to determine, generally, the cases in which bloodletting is applicable, Fauchier should have begun by investigating its effects in each particular disease ; not indeed, in a careless manner, but rigorously ; an immense labor, which would demand the lives of many industrious men.

We feel besides that a man, who places so much confidence in *a priori* considerations, will not be very exact in respect to particular facts. Thus Fauchier, after having combated generally

the opinions of the few physicians, who reject bloodletting in certain cases, in which he thinks it necessary, Fauchier, to support his own views, quotes the following facts, which I relate without abridging them :

“ So, because Madame C. J., attacked with pneumonitis truly inflammatory, was seventy years of age, her physician refuses to bleed her, and she dies on the fourth day. G. J. having the same disease, was not bled, because the physician was not called until the fifth day ; and the disease terminated by a vomica. A lady, attacked with an inflammatory sore throat, is not bled, or only a very little, because the catamenia were present ; and she dies suffocated !” &c. &c. (page 169). What facts ! What logic ! For we see every day persons, who have been very copiously bled, die of inflammation ; and in order that the quotations of Fauchier should be of any value, supposing his diagnosis exact, it would be requisite that the antiphlogistic treatment, more or less energetic, should be infallibly successful in cases of inflammation.

In a subsequent part of his work, touching the indications of yellow fever, the author says, “ if all those, who have seen yellow fever, were agreed concerning its course, its symptoms, its effects ; we might then understand its nature and decide upon the adoption or rejection of bloodletting,”

&c. (page 212). That is to say, Fauchier proceeds in the whole course of his work, *a priori*, as has been done, even to this day, by the most able men, who have considered therapeutics as a simple corollary of pathology. And what has been the result of it ? That, to this day, physicians remain divided upon important questions, such as derivation and revulsion ; questions which they principally endeavor to resolve, by the method of induction or *a priori* ; and that they scarcely agree, except on points which are admitted without any examination, or as established by long usage, which has nothing to commend it but time.

Freteau¹ proceeds no otherwise than his predecessors ; his point of departure is the same. Like them, he makes the indications of bleeding, in each disease, to result from its general effects, which he thinks he has determined. An excellent method if essay-making were the object, but which brings us to the infancy of therapeutics : since it can conduct only to probabilities, and not to certain results.

However, one of the first precepts of the author is, that the movements of nature must

¹ *Traité élémentaire sur l'emploi raisonné et methodique des émissions sanquines, avec application des principes à chaque maladie ; ouvrage couronné par la Société de Médecine de Paris, le 5 Juillet. 1814.*

be followed. But the justice of this precept, which physicians have transmitted from age to age, is in this way no better demonstrated, than if he had not stated it. (page 9). For if it means any thing, it undoubtedly is this, that if hemorrhage, for example, takes place in any affection whatever, we must seek to renew it by the same channels, or to establish it artificially. But in order that the utility of this practice should be, I will not say, demonstrated, but simply probable ; it would have been necessary to show, not by a few facts, but by a very considerable series, that patients in whom this hemorrhage occurred, recovered sooner, or in greater numbers, all else being equal, than those in whom it did not occur. And where is this demonstration found ? But suppose it admitted, we should, as I have just said, only have probabilities concerning the efficacy of bloodletting. For, who can affirm, without experience, that the effect, resulting from a loss of blood by the lancet or by leeches, will be exactly the same as that by a spontaneous hemorrhage ? Have not the authors, who have inculcated the precept under examination, themselves said that a few drops of blood from the nose were often followed with more relief than copious bleedings ?

After having combated the objections made to the doctrine of derivation and revulsion, we

shall develop, says Fréteau, a large body of rules, founded upon the most respectable authorities, and which moreover are adapted to reconcile all opinions. (page 19).

The reader will be astonished, undoubtedly, that in the nineteenth century, authority could have been invoked, in a science of observation ; without remarking that that, which we call experience, even now, is nothing but authority. In fact, to what authorities do those, most celebrated for the wisdom of their precepts, refer, unless it be to the practice of their predecessors, the superiority of which is by no means proved ; and the consequent results cannot therefore be considered, those of experience, properly so called. For true experience in medicine, as I have elsewhere remarked, (and as any one may be convinced, by what has preceded,) true experience in medicine can result only from the exact analysis of numerous facts, well ascertained, classed according to their resemblance, compared with care and counted. And of how many diseases has the treatment been thus investigated ? Let us not forget it then in future : if the experience, so justly scorned by Quesnay, is an uncertain guide in practice, it is that it possesses nothing of true experience but the name ; that it is, in truth, only the common usage, not justified by rigorous observation ; authority, in a word.

As the word experience, ill defined, has been an unanswerable argument for many physicians; it has been the same with the word *success*. Thus, in speaking of the period at which we must bleed, Fréteau cries, "Baillon, Riverius, Sydenham, &c. have imitated the example of Hippocrates, and obtained success!" (page 26). But how has this success been demonstrated; in other words, how has it been proved that the duration and mortality of a disease have been less under one method of treatment than under another?

Too often, it must be confessed, by the method of Fréteau, who himself believes it possible to demonstrate the ill effects of excessive bloodletting, by such facts as the following: "Casimir Medicus relates that, bloodletting having been employed towards the end of an acute fever, an cedema of the feet supervened, which resisted every remedy." (page 10). One would say, that many authors considered facts only as a sort of luxury, to be used as seldom as possible; and when they are used, the facts, which seem to indicate their love of truth, really amount to nothing. For supposing a fact well ascertained, accompanied with all the circumstances, all the details which make it valuable, supposing it to prove any thing, to be a sure guide to general principles; what can be done with statements like those just cited; where the au-

thor states neither the age of the patient, the period at which bloodletting was employed, the duration of the disease, the means used in conjunction with bloodletting, nor the condition of the organs at the commencement of the disease, &c. &c.

Let me not be charged with exaggeration; for even at the present time, particular observations are held valuable only in proportion to their brevity: and it is for this quality, chiefly, that facts transmitted from the ancients are so much admired. I will add, that the bare idea of *proving*, in pathology and therapeutics, according to the practice at the present day, by selected observations, even when sufficiently detailed; that this bare idea shows that the science of medicine is not considered by physicians, as they say it is, to depend entirely upon observation. Otherwise they would seek for truth in all the facts at their disposal, provided they were exact; lest by omitting any, they should arrive at false results: as in the physical sciences, one is cautious in suppressing any of the data of the problem to be solved, being well convinced that such suppression would render its solution either impossible, or false.

On account of the immediate communication of hemorrhoidal veins with those of the abdomen and pelvis, the application of leeches to the

anus and parts adjacent has important advantages, according to our author, in embarrassment, or inflammation of the viscera, &c. (page 73). That is to say, it is the common practice: and, as if the mere statement of it were sufficient, Fréteau does not trouble himself to prove its truth.

Most certainly the purely anatomical considerations, upon which this author relies, might and ought to warrant a trial of the application of leeches to the anus, under the circumstances stated: but, until decided by experience, the utility of the trial was problematical. We need then the result of experience on this subject to convince us, to prove clearly the utility of the practice in question; but it should be that true experience, which I have described: that is to say, it should show by exact facts, rigorously analyzed and counted, that the diseases in question were cured more frequently and speedily by applying leeches to the anus, than by applying them elsewhere. Up to this point, the author's precepts are bare assertions, and it is because the precepts of the present day in therapeutics are chiefly made up of assertions, that theory is said with so much truth, to differ essentially from practice.

By the same *a priori* considerations, the author still further indicates the cases, in which

leeches are preferable to venesection, (pages 94, 96) ; so that in his estimation, presumption, probability, indication and demonstration, are synonymous terms.

It would be idle to make any more extensive quotations from Fréteau, in order to judge of his method ; and I conclude with what he has said concerning pleurisy. It is asked whether a patient suffering pleurisy shall be bled from the arm, or from the foot ; and on which side, the affected, or the opposite. "Opinions," says he, "have been hitherto divided on this point ; but the voice of experience seems at last to have declared in favor of the doctrines of derivation and revulsion. Therefore, confirmed pleurisy demands derivative bloodletting ; that is to say, from the arm of the painful side. The practice of Triller may serve as a guide in this particular. In the sixth of his reported observations, he comments upon a violent pleurisy of the right side, which had existed with severity for three days ; blood was abstracted from the left arm which was not indicated. Triller bled the patient from the right arm and all went well." (page 235). Then follow two other observations of the same kind. The author's mind is so preoccupied with Triller's doctrines, that he does not perceive that two bleedings may be more efficacious than a single one ; and he draws a conclusion in favor

of Triller's doctrine. But, supposing the doctrine true, is a proposition in medicine to be established by two facts; especially when those facts admit of two different explanations? The question as to where bloodletting should be performed in pleurisy, it is evident, can be decided only by the method we have before laid down.

The course of Vieusseux¹ is like that of the authors we have criticised. After their example, he states in the first place its general effects, and thence deduces the indications for bleeding in particular cases.

He begins his review of the diseases, in which he thinks bloodletting indicated, with those of the head; confining himself, on this subject, to simple precepts; repeating what others have said, as if unimportant customs, and not a science, were the object of his studies.

Speaking of epilepsy, he says, "I have almost always used leeches at intervals," (to the anus undoubtedly), "and with success." (page 63). To this an answer readily presents itself; if you are sure that you have treated epilepsy more successfully with leeches, than without them, it must be, because, other things being equal, you

¹ *De la saignée et de son usage dans la plupart des maladies*, par Vieusseux, 8vo. 1805.

have cured a greater number of epileptics with them, than without them. If so, you must have counted the cases ; and why do you not state the number ? It would not have made your book much more voluminous ; and we should have demonstration in place of mere assertion.

“In croup,” says Vieusseux, “bloodletting should be prompt, because the affection is among the most rapid in its progress. The disease should be *prevented* : for, when once developed, it is rarely cured.” It must be prevented ! No doubt it is very desirable to prevent diseases ; but, in order to know what reliance can be placed on our means of prevention, it would be requisite, in the first instance, that the premonitory symptoms should be pointed out so clearly, as to leave no room for doubt. And who possesses such a knowledge of the premonitory signs of croup ? With regard to preventives in this disease, the only proof of their efficacy would be that in an epidemic, all other things equal, more of those who employed preventives, escaped the malady, than of those who did not. But this can be decided only in an epidemic, and by the method so often referred to.

In respect to the discrimination to be exercised in the use of bloodletting in malignant fever, the author says, “Those cases must be excepted, where an able practitioner, seizing the favorable

moment, decides at once to bleed : although not indicated, according to common usage. In such a case the physician acts as if from inspiration ; and genius rises above rules."

Behold, then, tact, inspiration, chance, transformed into genius ! For what is inspiration, or tact, if it is not chance ? What more is wanting to prove that Vieusseux trusts much to chance ; that he shows but little accuracy in appreciating facts ; and that he never imagined it possible to attain determinate results in pathology and therapeutics ! How can one, who regards medicine as a science, write on this subject as Vieusseux has done ?

Our author, as may easily be conceived, has not been very difficult as to particular examples ; and in adducing proof of this, I am embarrassed only as to a choice among the cases he states. Thus on the subject of abdominal diseases, which he thinks are often attended with gangrene, he says, "I have seen an instance of the alternate use of venesection and leeches in a female thirty years of age, who was subject to pain in the abdomen, and who suffered two or three days without fever and without tenderness on pressure. Suddenly the pain became very violent, and was accompanied with fever and vomiting. She was bled eleven times, and meanwhile had leeches to the anus twice, in the course of seven or eight

days ; she recovered rapidly, escaping suppuration, which should be avoided at any cost." (page 165).

Vieusseux considers this observation neither as short, nor as incomplete ; he gives it as if it were approved. Now I will ask the reader what is proved by an observation, relative to an abdominal affection, which contains no account of the form and volume of the abdomen, of the condition of the discharges, of the color of the matter vomited, of the expression of the face, nor of the state of the pulse, &c. nor of the changes which took place from one bleeding to another, &c. And this is the same author who declares, in his preface, that the facts remain. Unquestionably, they do remain : but for the most part to show how imperfectly observations have hitherto been made, in what contempt they have been held : and very little, it must be confessed, for the instruction of the reader.

The work of M. Polinière, which was crowned in 1826 by the royal society of Marseilles, is incontestably superior to the preceding. Particular observations are there met with much less incomplete, and in greater numbers. At the same time, an examination of some passages is sufficient to convince us, that the method of the author is not much more rigorous than that

of his predecessors ; like them, he lays down general principles with a view to deduce particular indications and rules of practice.

Having sketched in the first chapters the history of bloodletting, M. Polinière devotes the second to local bleeding, with a view of deciding upon what region leeches should be applied. On this subject he quotes Vitet, who was of opinion that they should be applied at some distance from the seat of the affection ; (page 28) ; and he is astonished to hear such language from one who takes observation and experience as the basis of his opinions, while it is from observation and clinical experience that we should be led to adopt a different practice.

I do not share the astonishment expressed by M. Polinière ; indeed I should have felt greatly surprised, if I had seen the same conclusions arrived at, by two men, who dignify their *a priori* precepts, with the name of experience ; for such experience is fallacious.

One of them, having seen some cases, in which more or less speedy relief has followed the application of leeches near the affected part, has decided in favor of this practice: the other, having witnessed an opposite practice and similar success, is in favor of applying leeches at a distance from the seat of disease. But such facts prove nothing : unless it be, that the application of

leeches at different points, does not prevent the recovery of the patients : so that the pretended experience of authors is worth nothing, and after all their assertions and denials, we are no further advanced than before. How then shall we proceed to settle the point in dispute? Evidently, as I have often remarked in like cases, by collecting a large number of facts accurately stated, relative to patients affected with the same disease ; some of whom shall have had leeches applied near the affected part ; and others who have had them at a distance from the seat of disease. Supposing the first to have recovered, all other things being equal, more speedily and in greater numbers than the second, the question will have been decided in favor of the application of leeches near the seat of disease ; and vice versa. How indeed can we reject a conclusion thus supported by evidence?

In entering upon the effect, which the irritation of leech bites may be expected to produce, "there are many cases," says M. Polinière, "in which the sole purpose of the application of leeches should be to cause a more or less prolonged irritation and a local fluxion. For instance, when it is our object to restore a suppressed catamenial, or hemorrhoidal discharge, experience teaches us that it is not to be attained by a single application of a large number of

leeches to the vulva or anus; but by irritating, by causing a fluxion towards the external parts, three or four days in succession, by means of leech bites. The effect is thus produced gradually, &c." (page 39).

Here again the author appeals to experience in support of his assertion. But the experience, to which he refers, is evidently tradition, custom, common belief; it is that almost worthless thing, which Quesnay denounced so vehemently; a compound of vague recollections; and not the rigorous expression of definite and closely analyzed facts: thus this precept of a man of talents must be considered as worthless.

On the subject of derivation and revulsion, M. Polinière readily shows, that the authors of these doctrines have thrown confusion, alike into their language and their rules in therapeutics. But how does he prove that derivation and revulsion are imaginary? He cites authorities; and among others, that of Pinel. He might as well have confined himself to a simple denial: for what is authority in medicine?

It is evident that to make this denial, with a full understanding of the subject, and to persuade others, would demand the labor which I referred to in the case of Quesnay.

Our author endeavors to determine the cases in which bleeding from the jugular vein is pre-

ferable to that from other vessels. "Clinical reports," he remarks, "may undoubtedly present us examples of cerebral inflammation, cured by the salutary influence of bleeding from the neck. But can we not oppose to this a still greater number of similar diseases, often of the most violent character, which have yielded, as if by magic, to an easier method of venesection? In order to establish the superiority of bleeding from the jugular beyond dispute, it must be proved by facts, that in a severe disease where bloodletting in the common mode had failed, the opening of the jugular was followed by an unexpected restoration." (p. 83).

Without doubt, this is indeed the real state of the question; but how shall we attain the proof which is demanded? Not, as the author proposes, by comparing two cases of cerebral disease: one of which, bled from the jugular, shall have recovered—whilst the other, bled from the arm, or foot, shall have died; and maintaining that the result of the latter case would have been different, if the patient had been bled from the jugular. For this does not admit of proof; since it may always be supposed that the resemblance between the cases supposed, is only apparent: and that the diseases were not exactly similar, so that the argument is of no value. On the other hand, let us suppose forty individuals hav-

ing a well marked cerebral affection, of the same duration and severity, to have been bled from the arm or foot; suppose forty others, affected with the same disease, and under similar circumstances, to have been bled from the jugular; suppose thirty cases of recovery among the latter, and only nine or ten among the former; the conclusion is then evident, that under the given circumstances, bleeding from the jugular is to be preferred. And this conclusion will be rigorous; for granting the impossibility, as I have before said, of appreciating each case with mathematical exactness, the errors will be the same in the two classes treated in different modes; these errors, then, will balance each other, and may be disregarded without sensibly affecting the result. Let the therapeutic question then, relative to bleeding, be what it may, it cannot be solved without the aid of the numerical method.

At first sight, nothing seems more easy and expeditious than a method, which dispenses with so much useless reasoning. Unfortunately this is by no means the case: for, as we have already seen, it presumes a comparison to have been made between a great number of cases of the same affection; in a part of which, the disease shall have been left to itself, so far, at least, as it can be done; in another part, certain modes of treatment shall have been instituted. This

method further presumes that the same therapeutic agent shall have been studied under the most diverse circumstances; that it shall have been given in minute and in powerful doses; at the onset of the disease, or at a certain period afterward; alone, or in conjunction with other means; in young and old subjects, &c. &c. And not only does the method demand much labor; but the requisite assemblage of facts, relative to any particular disease, is not made without much difficulty. All this, it must be confessed, has hitherto been imperfectly regarded by learned societies, who, in proposing prize questions, on bloodletting, for example, have expected candidates to traverse the whole circle of diseases, and to lay down rules for all cases. On their part, the candidates, partaking of the spirit of their cotemporaries, were not in the least embarrassed; and a single year, and sometimes even less, has sufficed them for the solution of problems, which, to be accurately solved, would have occupied the lives of many individuals. The result of this has been, that the authors of the prize essays, and all who have been engaged on the subject of bloodletting, have failed to settle definitively any one precept. Instead of aiming to make questions comprehensive, learned societies should have restricted the limits of the points to be investigated and discussed: and

they would have done themselves more honor, in my opinion, if, instead of proposing as a prize question, "To determine, by clinical observations, in what diseases the application of leeches is to be preferred to bloodletting; and when it is advantageous to employ both simultaneously:"¹ if, instead of questions like this, too comprehensive to be solved by one man, they had limited their inquiry to a rigid exposition of the effects of bloodletting in pneumonitis, for example, or in any disease whatever, but in one alone; for then only they would not have asked impossibilities.

The comments, made on the subject of bleeding from the jugular, are applicable to the author's remarks on bleeding from the arm, as compared with that from the foot: and I shall not dwell upon them.

In his sixth chapter he discusses the indications for bleeding, according to age, sex, temperament, &c. His precepts are founded on some facts favorable to his views; (bad logic, for one can thus prove any thing,) or upon the experience of the ancients; and we have seen before what is the character of that experience, founded, as it is, almost always upon tradition,

¹ Questions proposées par la Société de Médecine de Marseille, en 1825.

without proof. Besides, if it is remembered that the author was obliged to notice the influence of age in all diseases, in which bloodletting is practicable, it will be perceived that he has pursued the only course in his power.

In the second part of his work, which is the most considerable, M. Polinière makes the application of the principles, which he has laid down; beginning with inflammation of the gastro-intestinal mucous membrane. Before coming to particular facts, the author makes the following remarks: "I could easily have adduced a host of facts, in support of my propositions, on the subject of bloodletting. In a vast hospital, like that at Lyons, there is no want of facts; but such a mass would have encumbered my book, without adding to its usefulness. Being well persuaded, that extraordinary and rare cases ought not to occupy a prominent place in a work on practical medicine, that those of daily occurrence should be the object of study; I have made a selection of such histories of disease as may be considered a faithful expression, as a simple, clear representation of a multitude of analogous cases. I have therefore cited only three or four specimens of each disease, believing them sufficient to show my method of proceeding in similar cases." (page 203).

Without doubt, a few examples are sufficient

to exhibit the practice of M. Polinière in similar cases; but they are not enough to prove that it is good: and, supposing it to be good, to show how far it is so; and, in truth, this is the actual question. Does any one ask if I would have desired the writer to narrate, in succession, a hundred cases of any one disease? Assuredly, I should not: but I would have desired him to give a strict analysis of those cases: since, provided they were exact, he would by this analysis have proved something; but, cited separately, they prove absolutely nothing. For, let us not forget, if nothing is effected, in science, unless every proposition is rigorously demonstrated; so in therapeutics, nothing is effected, unless it is demonstrated, that, under certain conditions, a therapeutic agent produces this or that effect, has this or that influence on the course and termination of a disease, under given circumstances; and the most able physicians have, it must be confessed, forgotten little else than this very demonstration.

Let those, who engage hereafter in the study of therapeutics, pursue an opposite course to that of their predecessors. Let them not think that they have done any thing effectual, when they have only displayed their own theories, or stated what is done by the most celebrated physicians in such or such a case. But let them

labor to demonstrate, rigorously, the influence and the degree of influence of any therapeutic agent, on the *duration, progress, and termination of a particular disease*. Let them not forget that nothing is more difficult, than to verify a fact of this nature; that it can be effected only by means of an extensive series of observations, collected with exactness; instead of touching upon a boundless inquiry, let them limit the subject, that they may master it completely, and study it in all its aspects. Let them reflect that while this is the only means of being useful to science and to mankind, it is at the same time the only source of true fame to the student in therapeutics.

I shall not pursue any further, the examination of the treatise of M. Polinière: enough has been already done to give an idea of his method; and I shall refrain from any comment whatever on later researches: so that no one may be at liberty to suppose, that in the criticisms I have made, I have had any other design than to discharge a duty.

THE END.

APPENDIX BY J. J.

THE results attained by the foregoing inquiry will, no doubt, surprise many, if not most, medical men. They certainly do not accord with my own previous impressions in various respects. I had believed, for instance, that bloodletting after the third or fourth day in pneumonitis was not often useful, and that sometimes it was injurious; but that on the first, second and third, and perhaps on the fourth, it both mitigated the disease and shortened it. It was my practice to bleed freely in the first instance; at least more freely than was done at the hospital la Charité, where the observations given in the first chapter of this work were made. Accordingly, when I saw those observations three years ago, published, as they then were, separately, it appeared to me very certain that a more copious bleeding on the early days would have given different results. After reading this work, where the second chapter shows that very little, if any thing, more

was gained by a copious bleeding in the first instance, my faith was shaken; though I still thought some exceptions might be taken to the conclusions, at which M. Louis had arrived. Particularly it did not appear that he had had many opportunities to order these bleedings in the three first days of the disease. But, instead of urging objections to his conclusions, it seemed best that I should inquire accurately how far the facts within my own reach would confirm or contradict them. On this account I determined to examine the case-books of the Massachusetts General Hospital. It is the object of this appendix to give the results of that examination. Before giving these results, it may be proper to make some statements respecting this hospital, and the manner in which its records are kept, so that it may be seen on what authority my facts are grounded.

This hospital was opened in 1821; but it was not ready to receive many patients till 1824; since that time it has received annually about three hundred medical patients.¹

From the day, on which the hospital was opened, the cases under the care of the physicians have been recorded at the bedside. After a little experience the following course was adopted, and has been closely adhered to. On the entrance of a patient, the house physician (usually

¹ The whole number, from 1824 to 1834 inclusive, is 3291, exclusive of those remaining Jan. 1, 1835. Of these 257 have died, being 1 in 12 4-5.

a medical pupil in his third year, and always a resident in the hospital), collects and writes down the history of the case and the actual state of the patient. The physician visits the hospital every morning, and examines every patient daily. He dictates aloud the record of the day at the bedside of each patient, and the prescriptions, if there be any. All this is recorded at the moment by the house physician, in a first book, or journal, where the record goes on continuously from one patient to another. Before the visit of the following day, the record thus made is transferred to a case-book, under the head of each case; a process similar to that of posting from a mercantile journal into a ledger. This case-book is carried round by the physician in his daily visits; and, as he arrives at the bedside of each patient, he opens to his case. At this time there are sixty-eight case-books filled in this manner; each book being a folio, thirteen inches by eight, and containing upwards of two hundred and fifty pages. The whole number of cases cannot be less than three thousand five hundred. It will be conceived at once that the records are not short. They probably are as minute as most records of the kind. It has been the general practice to inquire into the state of all the functions; and to note all such as were disordered; so that, where nothing is distinctly said of a function, it is to be presumed that it was regular. It is not true, however, that this practice has been undeviatingly pursued. The results of examinations after death, where these have been

permitted, have been recorded in the case-books, though not always with the precision which modern science demands. In all points, however, the records have been made with increasing precision from year to year.

Among the numerous cases in this hospital, it might be imagined that a large number would be found of each of the diseases, to which M. Louis has referred ; and that cases might be selected, which would compare with his. But it is not entirely so. Erysipelas of the face has shown itself at times, and on two or three occasions it has been a formidable disease, causing some deaths and great anxiety. But it has not occurred in those previously healthy ; with two or three exceptions it has been in those only, who were already patients of the house, either surgical or medical. The results of treatment cannot therefore be compared with those of our author. Besides, it has not been a common practice among us, or certainly not with me, in the hospital, nor elsewhere, to take away blood in this disease, unless the subject was quite vigorous, or the pulse peculiarly strong and hard. I will venture to add that the following has seemed to me the most successful treatment of this disease, when seen very early ; viz. first, to clear the bowels by a cathartic, and, if specially indicated, the stomach by an emetic ; second, to administer the cinchona, or the excellent substitute we now have, the sulphate of quinine. These are given in as large doses as the patient will bear. From twelve to twenty-five grains of the sulphate in twenty-four hours

will generally suffice. That the dose is sufficient will be known by a buzzing in the ears; when this occurs, the dose may be diminished a little. Third, covering the parts much of the time with a thin linen, which is kept moistened with either diluted alcohol, or a solution of acetate of lead; (two drachms to a pint of water). I cannot say which of these lotions has proved most useful. If this treatment is commenced on the first appearance of the local disease, I think there is a very good chance that the disease will cease to spread, and that the diseased part will be covered by scales on the fifth day. My experience, however, has been mostly in private practice, because my hospital patients have usually had other diseases which prevented the fair trial of this method of treatment.

As this treatment has been pursued, I believe, for a century by some judicious practitioners, though not perhaps very generally adopted, I think it well to suggest it for future trial, hoping that the merit of it may be decided upon the principles of the numerical system.

As to angina tonsillaris (*l'angine gutturale*, as denominated by M. Louis,) our records will not furnish any materials, which can be added to those of our author. If we except angina maligna, or the sore throat, belonging to scarlatina, and one other affection to be mentioned directly, we do not often see cases of acute inflammation in the tonsils and palate, which are so severe, or so hazardous, as to call for bloodletting. We do indeed some-

times apply a few leeches in such cases with a hope to abate the severity of the disease, but with what success I am not prepared to say. There is however an affection of the tonsils, which tends to suppuration, and which may usually be distinguished at an early period, though not at its very commencement, by two symptoms. One is the peculiar kind of dysphagia; the patient swallows as if he were lifting the morsel, or the liquid over a sore part, and makes a peculiar contortion of countenance on the side affected. If both sides be severely affected at once, which is very rare, he is scarcely able to swallow. The second symptom is a peculiarity in the speech. The patient speaks with some, and often with great difficulty, but is not hoarse; the voice sounds as if there were an obstacle to its egress, not to its formation, such as would happen if he were speaking, when a large mouthful of food had arrived at the entrance of the fauces and was suspended there, so as to impede his utterance. Where these symptoms have shown themselves, I have known how much trouble to anticipate for the patient, and early in my practice I tried bleeding and other remedies to arrest the disease. But, it is many years since I learned that, after the two symptoms, above described, had occurred, my efforts were never, or almost never, successful. And, not having found the disease to terminate fatally, though it occasions much distress, I have abstained from great evacuations of any kind, as causing a useless reduction of the strength.

Pneumonitis, a name which I use as equivalent to pleuro-pneumonia, is then the only one of the three inflammatory diseases, of which our records will furnish cases for comparison with those referred to in the preceding work.

It might be supposed that in this cold and variable climate pneumonitis would be a very frequent disease, and that, in the cold seasons, no small portion of our hospital patients would be affected with it. But it is not so; especially it is not so among adults; and, with rare exceptions, persons under eighteen are seldom admitted as medical patients at our hospital. I had remarked the rarity of the disease in private practice before our hospital was erected. Since that time, we have had very few patients with it, except in those years when influenza has prevailed. In those years it has occurred in many persons, not as a part of the epidemic disease, yet as an indirect consequence. Persons exposed to the inclemencies of the weather, and more or less engaged in labor, such as domestic servants particularly, have had pneumonitis come on, as a distinct disease, in a period of one to four weeks after the commencement of the influenza. In these instances, whether the first disease has, or has not confined the patient to the house, the second has commenced with distinct chills, more or less prostration, and pain on one side of the thorax, followed within two days by cough and bloody or rusty sputa.

M. Louis, wishing to take only cases of pure pneu-

monitis, has rejected those which were preceded by catarrh. He would not therefore admit the cases, which I have just described. In the selections which follow, I have admitted such, because without them I could furnish so few as not to be worthy attention; but I have stated in each instance the preceding disease, so that the reader may know how much value to attach to the cases. I have rejected all cases, in which there was reason to believe that tubercles existed; and for the most part, if not always, those in which there was any other want of soundness in the health at the access of the pneumonitis, except those already referred to. I presume that M. Louis rejected the cases in which catarrh had preceded, because those cases might not terminate so quickly as others. As, in fact, our cases have generally terminated at an earlier period than those in Paris, I have thought no objection would be made to them on that ground. I may remark, on this point, that I cannot attribute this difference to our climate, nor to the medical treatment, properly speaking. Nor will I decide as to the cause. But I suspect that the cause is, that in Paris the hospitals are very cold in winter, while ours is uniformly warm, though well ventilated. In the day the temperature of our wards is rarely lower than 65° Fahrenheit; and I believe that it is rarely lower than 50° in the most severe nights. When it has been very low in the night, I have usually discovered it by an increase of disease in several patients on the next day.

The table I. contains a statement of many particulars in all the cases, which I have selected from our case-books, being in number thirty-four. I might have added more cases, but none which would not have been subject to some material objections, as regards the points under consideration, or deficient in particulars of essential importance. Thus, in some instances, the previous health of the patients had shown a manifest tendency to phthisis, or the actual existence of this or some other important disease. Some of the cases I have given may not be regarded as altogether free from objections on this score. In other instances the period of attack, or the symptoms at an early period were so imperfectly ascertained, that the cases would not justify any such inferences as we look for in the present inquiry. Fatal cases have been omitted entirely in the table.

TABLE I.—Analysis of 34 cases of Pneumonitis treated at the Mass. General Hospital.

Case.	Entered Hosp.	Disease began.	Age	Sex.	Side affected.	Previous health.	The days of disease on which bled.	Quantity of blood taken.	Day of discharge on which vesicated.	Internal Medicine administered.	Day of discharge on which convalescent.	Relapse.	Disch'd from Hospital.
I.	Ap. 19, 1825	Apr. 17	30	m.	right	Injured left side by a fall two weeks previously; nearly recovered.	3d	3 xiii. faint from bleeding.	3d	Cathartic. Colicium 2 days, 5 as a d. v. Hyd. Sulph. 19th to 26th; mouth sore 25th April.	11th	None	May 3d
II.	May 7, 1825	May 4	45	f.	r.	Catarrh, cough, hoarseness for two days.	4th	3 vii. faint	4th	Pil. H. S. C. 7th to 10th; mouth sore 10th May.	8th	None	May 18
III.	Feb. 17, 1826	Feb. 16	20	f.	left	Not stated; probably good.	2d 4th	3 xii. 3 xvi.	2d, 4th, 10th, 21st, 22d	Cath'c. Colchicum. H. S. 18th to 22d Feb.; mouth sore on 22d; emetic on 21st Feb.	13th	March 8 convalescent March 15	March 18
IV.	Feb. 17, 1826	Feb. 10	26	m.	r.	Catarrh 2 days before attack.	9th 10th	3 xiv. 3 xiii.	8th, 10th	Pil. H. S. C. 17th to 23d Feb. mouth not sore. Colchicum 21st to 23d.	15th	None	March 1
V.	Mar. 6, 1826	March 3	17	m.	l.	Not stated; probably good.	4th	3 xvi.	5th	Cath'c. Pil. H. S. C. 7th to 9th March; mouth not sore.	9th	None	March 22
VI.	May 3, 1826	April 28	25	f.	l.	Not stated; probably good.		Not bled; an attempt made, she became faint.	6th 7th	Cath'c's. H. S. 3d to 8th mouth a little sore from the 6th May. Colchicum 4th and 5th May.	15th	May 21 slight after exertion	May 29
VII.	Feb. 26, 1828	Feb. 25	33	m.	r.	In Hospital last month for abscess in axilla; cough then and since.	2d	3 xvi. slight faintness.	2d	H. S. from Feb. 26th to March 2d, mouth sore Feb. 28th. Colchicum Feb. 27th to March 1st. Cath'c's.	9th	March 8 slight after exposure a broad.	March 18

REMARKS ON THE CASES.*

I.—The fall caused a week's confinement, the second week he worked a little; probably was tender. The pain in side was relieved at once by venesection and pulse less frequent next day; cough increased. Sputa less colored after 20th April; not at all after 23d. P. 96 to 72 on 19th to 23d; less frequent afterwards.

II.—Attack on 4th distinct; disease not severe; a leucorrhœa kept her in hospital two days. Pain in side nearly gone on the day after v. s. and blister; and p. reduced from 90 to 72, and less from that time. Sputa not colored after 19th May.

III.—Influenza prevalent this season. This case, like many others this season, was complicated with gastric affection. Convalescence on 13th was complete, though she relapsed 8th day afterwards. Pain relieved day after 1st v. s., but p. more frequent, cough worse; cough better and sputa less bloody after 2d bleeding; relief of gastric symptoms after emetic. Vesication after relapse on 21st and 22d day of disease gave relief as to pain and cough. P. 68, on 17th Feb. (perhaps an error in record); 96 on 18th; 128 on 19th; 96 on 20th; 120 on 21st; 96 on 22d; 104 on 23d; 96 on 24th; 100 on 25th; 84 on 26; 72 on 27th; and again accelerated before relapse on 8th March, and then for three days.

IV.—No gastric affection. Pain less after 1st vesication but not other symptoms. Some relief as to dyspnœa and p. fell from 104 to 96 after 1st v. s. More relief after 2d v. s. and two days after p. 84, and after this day 72, with steady convalescence. Sputa altered favorably from 2d v. s.

V.—On 16th gum-boil, which detained him in Hosp. V. s. was before his entrance though on same day, so that effects could not be known. Pain relieved by vesication; and amendment from March 7th progressive.

VI.—Some relief as to pain and respiration after 2d blister. Colchicum caused too much vomiting, but amendment began the following day, May 6th.

VII.—It may be doubted whether the previous health was sufficiently good. One day after v. s. pulse was lessened, also relief of pain (attributed to vesication by patient), dyspnœa also less, and sputa changed from red to rusty. Diarrhœa and colic, induced by colchicum, ceased on its omission.

* These remarks constituted the last column in the table. It was necessary in printing to place them separately, below. The numerals, I. II. &c. refer to the cases above.

Case.	Entered Hosp.	Disease began.	Age	Sex.	Side affected.	Previous health.	The days of disease on which bled.	Quantity of blood taken.	Day of disease on which vesicated.	Internal Medicine administered.	Day of discharge on which convalesced.	Relapse.	Disch'd. from Hospital.
VIII.	May 8, 1828	May 7	22	m.	left and right 6th day.	Catarrh for two weeks.	2d 4th	5 x. faint 5 xii.	4th, 6th, 7th	Pil. H. S. C. 8th to 18th May, mouth sore on 16th. Colch. 9th & 10th, and on some other days.	13th	None	May 26
IX.	Oct. 19, 1828	Oct. 5	22	m.	l.	Not stated; probably good.	19th 16th	5 x. & 5 xii. 5 iv. by cup-plug	16th, 19th	Hyd. S. from 20th to 28th Oct. not every day; mouth sore 25th. Colchicum 22d to 28th both with opium.	26th	None	Nov. 11
X.	Jan. 24, 1829	Jan. 17	35	m.	both.	Not stated; probably good.	2d or 4th day, but the exact day and quantity not known. It was before entrance	5 xxxiv,	9th	H. S. 25th to 28th Jan. mouth not sore. Colchicum 25th to 27th.	14th	None	Feb. 11
XI.	Feb. 23, 1829	Feb. 22	28	m.	r.	For three weeks head ache, cough, anorexia; but kept at work.	2d	5 xxxiv,	4th, 8th, 18th	H. S. Feb. 23d to March 3d; mouth sore Feb. 26. Colchicum Feb. 23 to Mar. 3, but not every day. Cath'c.	13th	None	March 21
XII.	May 16, 1829	May 14	22	m.	r.	Not stated; probably good.	2d 3d 9th 11th	before entrance, quantity not known 5 xxx. 5 xxx. 15 leeches	3d, 5th, 8th	Colchicum May 16th to 23d, omitted one day. H. S. 17th to 24th; mouth sore 25th. Cath'c.	15th	None	May 30
XIII.	Dec. 10, 1831	Dec. 16	36	f.	l.	Influenza began about Nov. 27. She entered for this Dec. 10th; was nearly well Dec. 15.	1st	5 xii. slightly faint	1st	Pil. H. S. C. 16th & 17th Dec.; mouth not sore. Cath'c.	4th	None	Dec. 28
XIV.	Dec. 14, 1831	Dec. 11	16	m.	r.	Catarrh, probably influenza, a week or more before the attack.	5th	5 xii.	5th, 7th	Pil. H. S. C. Dec. 15th to 18th; mouth sore 19th. Cath'c. Opiales.	15th	None	Jan. 14, 1832

VIII.—Though catarrh preceded, the attack on the 8th was formal and distinct, marked by chills, &c., as in the other cases. Pain relieved and sputa less bloody day after 2d v. s. P. 108 to 96 till 14th May, then 84; 15th 72; 16th to 19th, 72 to 60.

IX.—This case was complicated perhaps, or not quite clear; and the commencement not exactly marked. P. 88 to 96 from 20th to 25th; 84 from 26th to 28th; 72 on 30th Oct. Softer but not slower the day after v. s.; then 96. Two days after v. s. 90. Sputa not noted as discolored later than the day after v. s. Delirium 21st and 22d days of disease.

X.—This man was severely sick and had a slow convalescence. Respiration less rapid and better after vesication. P. 96 from Jan. 25th to 28th, though otherwise mending on 28th; 90 on 29th; 72 on 30th; and from that day mending in every way.

XI.—Was mending decidedly Feb. 26th (5th day), but relapsed a little on 27th. The vesication 18th day of disease was for a very trifling complaint in the side, other symptoms all good. Sputa less bloody two days after v. s. and a sense of relief immediately on v. s., and the same continued next day. P. 104 on 23d; 96 on 24th to 27th; 108 on 28th; 84 on March 1st; 60 on 3d; 60 to 48 from 5th to 7th; never more than 48 afterwards.

XII.—May 22d (9th day) grew much worse, but greatly relieved by v. s. Severe strangury followed the vesication, which kept him back till 27th. May 16th, p. 96; 17th, 120, though relieved as to cough and dyspnoea; 18th to 21st, 84 to 88; afterwards not below 96 till 28th, being kept up by strangury. On 28th out of bed and dressed and almost free from pectoral symptoms, but p. not stated.

XIII.—The sudden relief in this case may serve to throw some doubt over it. The case will be found in detail, so that the reader may judge of it.

XIV.—Convalescence was retarded by very sore blisters, and he was detained in hospital by a boil. P. 84 on 15th, before v. s. and the same to 18th, though softer after v. s. Sputa better and less abundant from 16th. On 19th, p. 96, with delirium, without increase in pectoral symptoms then or afterwards; 84 on 20th; 80 on 21st; 72 to 60 on 22d to 25th; never increased afterwards.

Case.	Entered Hosp.	Disease be- gan.	Age.	Sex.	Side affect- ed.	Previous health.	The days of disease on which bled.	Quantity of blood taken.	Day of Dis- ease on which vesicated.	Internal Medicine ad- ministered.	Day of dis- charge on which convalesc'd.	Relapse.	Disch'd from Hos- pital.
XV.	Dec. 17, 1831	Dec. 13	42	f.	r.	Cough for more than a fortnight; pro- bably catarrhal. How- ever, such as belong- ing to the critical period.	7th	No V. S. Six leeches to head.	4th, before entrance, 13th	Cath'cs. Opiates. Pil. H. S. C. 17th to 31st Dec. but not con- stantly. Mouth very little sore on 22d, but soon well again.	17th	None	Jan. 23 1832.
XVI.	Dec. 23, 1831	Dec. 22	23	m.	r.	Influenza formore than a week.	1st 4th 8th	5 xx. and 5 xxvi. be- fore entrance. 5 xii. 5 xvi.	2d, 4th, 7th	Hyd. S. on 22d and 31st Dec. and from 29th to 1st Jan. then mouth a little sore. Colchi- cum 24th to 29th. Cath'cs.	14th	None	Jan. 10 1832.
XVII.	Dec. 25, 1831	Dec. 20	30	m.	left.	Influenza began 4 weeks before, and had not left him, though never confined by it	6th	5 xxii.	6th	Emetic. Cath'cs. Pil. H. S. C. 25th to 30th Dec. when mouth sore.	13th	None	Jan. 10 1832.
XVIII.	Dec. 30, 1831	Dec. 29	30	m.	l.	Influenza for a week.	2d 3d	5 xxii. 5 xv.	2d, 3d	Pil. H. S. C. 30th Dec. to 4th Jan.; mouth not sore. Ca- thartics.	8th	None	Jan. 10 1832.
XIX.	Jan. 9, 1832	Jan. 8	24	m.	l.	Influenza, slight, for a week.	2d 3d 5th	5 xiv. faint. 5 xvi. some- what faint. 5 xii. some- what faint.		Emet. before en- trance, 8th Jan. H. S. 9th to 19th; mouth sore on 19th. Colchi- cum 10th to 14th. Ca- thartics.	12th	None	Jan. 25
XX.	Mar. 29, 1832	Mar. 28	33	m.	r.	Had influenza early in winter, and slight cough since that.	3d 4th 7th	5 xx. 5 xxiv. 5 viii.	3d	Pil. H. S. C. 29th March to 31 April, when mouth was sore. Colchicum April 3d to 5th.	11th	None	April 25
XXI.	July 20, 1832	July 20	28	m.	r. perhaps both.	Not stated; pro- bably good.	1st 2d 3d	5 xx. and 5 xii. 5 xii.	1st, 2d, 4th, 5th, 6th, 7th	Emet. 20th July. Cath'cs. H. S. C. 21st to 25th July. Merc. fu- milg. 21th; mouth sore 25th; Ant. Tart. 26th, 22d and 23d; Colchie- um 23 to 25th; Opiates.	13th	None	Aug. 15

XV.—Much better Dec. 24th; increased pain in side, though otherwise not worse on 25th (13th day); pain relieved on 26th; took bread 28th (16th day); yet pain left her slowly. Menorrhagia Jan. 11th, which detained in Hosp. P. 120 to 96 from 17th to 22d Dec.; 84 from 23d to 28th; afterward not accelerated.

XVI.—On 22d Dec. there was a rheumatic attack on right knee some hours before chills and pain in side; the pneumonitis was however strongly marked and severe. The disease was more mitigated than shortened by v. s. P. 76 to 96 on 23d to 25th Dec.; 84 on 26th; 72 on 27th to 30th; then 88 to 76 till Jan. 3d, probably from mouth getting sore, for otherwise mending; 72 Jan. 4th, and afterwards less. Sputa slightly colored till Dec. 30th. Pain in side relieved by v. s. and further by vesication 25th Dec.

XVII.—Very marked relief by v. s. as to dyspnoea, pain &c.; p. 120 on 25th Dec.; 88 on 26th; 84 on 27th; 60 on 28th; afterward not more while in bed. Sputa colored till 28th. Suffered two days from soreness of mouth.

XVIII.—The pain, almost beyond endurance, much relieved by first v. s. Jan. 2d; after 2d v. s. and two blisters free from pain except on coughing, and after Jan. 2d no pain. No color in sputa after Jan. 1st.

XIX.—Pain and dyspnoea relieved by first v. s. Pain nearly gone on Jan. 11th, day after 2d v. s. and 2d vesication. On evening of 11th taken out of bed in great sweat, then chill, increase of pain, &c.; relief from pain next day after 3d v. s. After Jan 16th sputa not colored. P. 133 on 9th; 120 on 10th and 11th; 104 on 14th; 90 to 60 on 15th to 19th.

XX.—Erysipelas of face occurred on 10th April; of which he had recovered 15th April. Distinct relief as to sensations by first v. s. and sputa less colored next day; no such amendment after 2d, but similar relief after 3d; on April 6th sputa entirely free from color. P. 100 on 29th March; 84 on 30th; 120 on 31st, but this when sitting up; and, in same state, 116 on 1st April; 96 on 2d and 3d; 88 on 4th; 72 on 5th; 48 on 6th; and not more than 72 afterwards.

XXI.—Some relief, temporary, from 1st v. s.; more decided relief after 2d and 3d; the 4th was for dyspnoea and headache, also with some relief; p. 96 on 20th and 21st July; 108 on 22d to 24th; 96 on 25th and 26th; 84 on 27th; 72 on 28th and afterwards; sputa not free from color till 28th (3th of disease), but never very bloody.

Case.	Entered Hosp.	Disease began.	Age.	Sex.	Skin affected.	Previous health.	The days of disease on which bled.	Quantity of blood taken.	Day of disease on which vesicated.	Internal medicine administered.	Day of discharge on which convalesced.	Relapse.	Disch'd from Hospital.
XXII.	Nov. 6, 1832	Nov. 3	18	m.	right	Slight cough for two weeks.	1st 4th 5th	Twice, before entrance; quantity not known. 3 viii. somewhat faint. 3 xii. a little faint.	1st, 5th, 10th	Emet. 3d Nov. H. S. 6th to 15th; mouth not sore. Antim. Tart. 8th to 15th. Cathartics.	13th	None	Nov. 27th
XXIII.	Mar. 31, 1833	Mar. 23	23	m.	r. perhaps both.	Some disorder in digestive functions through the winter; cough some days before present attack.	4th 5th 6th 9th 11th	3 xxii. slight faint. 3 xvi. slightly faint. 16 leeches. 16 leeches. 3 xii.	Before entrance; also 8th	Emet.; Cathart.; Mar. 29. H. S. copious. Antim. Tart. April 1st to 6th. H. S. copious. 6th to 11th; mouth not sore.	17th	None	Apr. 29th
XXIV.	Jan. 31, 1834	Jan. 27	31	m.	r.	Well. Had pleurisy in Hosp. some years since.		Not bled.	5th, 6th, 18th	Pil. H. S. C. Jan. 31st and Feb. 1st. Colchicum Feb. 21 to 4th, also 13th to 16th.	12th	On Feb. 12th after exposure. Convalescent on 19th.	Mar. 3d
XXV.	Feb. 5, 1834	Jan. 23	35	f.	r.	Never very good.	14th 16th	Freely, before entrance, quantity not known. 3 xvi.	14th, 15th, 22d	Colchicum with op. Feb. 6th. Emet.; Cathartics.	24th	None	Feb. 22d
XXVI.	May 3, 1834	April 28	37	f.	r.	Subject to cough for some years; cough last winter; also painful menstruation and leucorrhoea.		Not bled.	8th	Emet. and Cath'c before entrance. Antimon. Tart. on May 3th to 10th.	14th	None	May 20th
XXVII.	May 11, 1834	May 8	21	m.	left	Not strong before puberty; getting robust for 5 years past.	5th 6th	3 xiv. faintness. 3 xiv. faintness.		Emet. and Cath'c before entrance. Antimon. T. May 12th to 17th.	10th	None	May 21st
XXVIII.	May 14, 1834	May 7	20	m.	r.	Subject to dyspnoea and palpitation for several years.	8th	3 xx. faintness.		Antim. Tart. 14th May. Ipec. and Op. 15th to 17th. Cath'cs.	13th	None	May 21st

XXII.—V. s. on 3d Nov. gave some relief, but pain in the side had again become severe on 6th and was little relieved on 7th; the v. s. that day gave much more relief. P. 120 on 6th to 9th; 100 on 10th; 72 on 11th and 12th; 84 on 13th; 60 afterwards. No color in sputa after 13th.

XXIII.—Some relief in the side after first v. s.; less relief on 2d v. s.; none from first leeches, but much after 2d; some from 3d v. s., but pain continued to 18th. P. 110 on 31st March; 96 on April 1st; 84 on 2d; 72 on 3d to 7th; 96 on 8th; 82 on 10th; 68 on 13th; after this not more than 60. Sputa not free from color till 6th. Convalescence retarded by disease of stomach, &c.

XXIV.—Detained in Hospital partly by otitis. Feb. 3d pain nearly or quite ceased. Sputa little colored on 3d, but not quite free from color till 8th. P. 108 on 1st; 96 on 2d; 66 to 60 from 3d till relapse on 12th, then a little quickened for two days.

XXV.—This case ought not, perhaps, to be admitted. The health was never very good. Though the attack was well marked, and at entrance the disease fully shown by physical and rational signs, yet she kept at work till the day before entrance (13th day of disease). P. 116 on Feb. 5th to 7th; 96 on 8th to 10th; 84 on 11th; not more than 72 afterwards. Sputa colored on 5th and 6th, but not afterwards. She experienced relief from the 1st v. s. by her report. Two days after 2d v. s. and 2d vesication there was marked relief, and from this time the amendment was progressive in all essential points.

XXVI.—Perhaps an improper case owing to previous ill health; but it was well marked in the attack and progress, though not severe. P. 112 on 5th May; 100 on 6th; 84 on 8th to 10th; 72 on 11th. Sputa not colored on 7th nor afterwards.

XXVII.—This case was not very severe. P. 88 on May 11th and 12th; 90 on 13th; 60 on 14th; 46 on 15th and afterwards. There was well marked amendment from 14th. Sputa not colored after 14th, (7th day of disease). The diet is not noted, and convalescence might perhaps have been marked on the 9th day.

XXVIII.—This case not severe. P. 100 on May 14th; 96 on 15th; 80 on 16th and 17th; afterwards not noted.

Care.	Entered Hosp.	Disease began.	Age.	Sex.	Side affected.	Previous health.	The days of disease on which bled.	Quantity of blood taken.	Day of disease on which vaccinated.	Internal Medicine administered.	Day of dis-convalescence.	Relapse.	Discharge from Hospital.
XXIX.	May 22, 1834	May 21	20	m.	left	Never robust; not very well for a fortnight; some diarrhoea with griping on 20th and 21st.	2d 3d	5 xxxii. 5 xii.	2d	Cath'cs. Colchicum with Ipec. 21th to 30th May.	10th	None	June 4th
XXX.	Oct. 22, 1834	Oct. 17	29	f.	right	Well.	4th	Quantity not known before entrance.	3d, 7th, 9th, 19th	Cathartic on 18th (Oct. Antimon. Tart. 23d to 29th.	13th	None	Nov. 19th
XXXI.	Dec. 1, 1834	Nov. 28	21	m.	l.	A cough for a fortnight.	4th	3 viii. a little faint.		Emet. and Cath'cs before entrance. Antim. Tart. Dec. 2d to 20th.	12th	None	Dec. 29th
XXXII.	Dec. 15, 1834	Dec. 9	28	m.	l.	Well, but not strong.	3d } 4th }	Before entrance, quantity not known; probably 3 xxx. 7th, 10th	Twice after entrance 3d before entrance; on 23d; Cathartics.	Antim. Tart. Dec. 16th to 22d. Emetic on 23d; Cathartics.	16th	None	Jan. 5th, 1835.
XXXIII.	Feb. 8, 1835	Feb. 4	19	f.	r.	Slight cough for a week.	4th	3 xvi. before entrance.	10th	Antimon. Tart. on Feb. 9th to 19th.	14th	None	Mar. 7th
XXXIV.	May 10, 1835	May 3	47	m.	r.	Catarrh much of the time in winter, but not this spring.		Not bled.		Antim. Tart. May 11th to 14th. Ipec. and Op. 14th. Cathartics.	15th	None	May 27th

XXIX.—Pleurisy well marked; pneumonic symptoms slight. P. 108 May 22d; 116 on 23d; 96 on 24th; after this 72 and 60. Sputa a little colored on 22d and 23d, but not afterwards.

XXX.—Convalescence slow on account of some trouble in digestive organs and a lingering pain in side; it was for this she was vesicated on 19th day of disease, though otherwise doing well. P. 108 on Oct. 22; 92 on 23d; 84 to 76 on 24th to 28th; afterwards natural. Sputa not much colored after 25th, and not at all after 28th.

XXXI.—The attack on Nov. 28th very well marked. Pleuritis well marked, pneumonic symptoms not so strongly. P. 96 Dec. 1st to 3d; 76 on 4th; 60 afterwards, except one day during convalescence under gastric troubles. From these troubles convalescence slow. Sputa colored till 4th, not afterwards.

XXXII.—The stomach was habitually feeble, and this cause delayed the progress of the case throughout. The v. s. was reported to have afforded decided relief; but in consequence of it, probably, the patient was very feeble when he entered. P. 110 Dec. 15th and 16th; 96 on 17th; 84 on 18th; 72 on 19th. Not more afterwards, except on casual irritations, but feeble and easily quickened. Sputa colored at entrance, but not after 17th.

XXXIII.—Case not severe, though not the mildest. P. 120, Feb. 8th; 96 on 9th; 84 on 10th and 11th; 72 on 12th, and not more afterwards, except once under excitement of mind. Sputa never much colored after entrance, but were mostly of a light straw-color rather than rusty till 16th Feb.

XXXIV.—Case not severe. P. 100 on May 10th; 88 on 12th; 76 on 14th and 15th; not less at any time after; but he seems to have been sitting up at most of the examinations and probably had a frequent pulse always. Sputa a little colored on 12th, but not afterwards.

Remarks on Table I.

Before making any observations or calculations upon the contents of this table, some *preliminary remarks* must be offered. And, first, great care is taken at our hospital to fix the date, at which the disease in every case has begun. It is not probable that we are equally successful in this respect in all instances. There is no doubt that we are more so in acute, than in chronic diseases; at least, where the patient has not been sick more than a week at his entrance, and where he retains the full exercise of his mental powers. Both these conditions usually exist in cases of pneumonitis. I do not doubt that the period of attack is accurately stated in almost every case in this table; and I know not any reason to doubt it in regard to any one case. Indeed, several cases were excluded from this table merely because the periods, at which they commenced, had not been well ascertained. As to all events, which occurred in the hospital, the dates may be fully relied on.

The period of convalescence is fixed on the principles laid down by M. Louis. In two or three cases only the period of taking food was not noticed on our records; but then collateral circumstances showed, when it must have been taken; and I have been cautious in each of those instances not to fix on too early a day. In all cases, except the thirteenth case, which will be explained hereafter, the febrile affection had been subsiding two or three

days at least before that, which is mentioned as the day of convalescence. As these cases occurred before we had regarded the taking of food, as marking the period of convalescence, there could be no inducement to indulge the patient too early on that account. I feel assured that my colleagues, as well as myself, have always been cautious not to allow a premature indulgence in the use of food, and in these thirty-four cases there was only one who appeared to suffer from food on the first days, on which it was allowed; and that was a patient, whose digestive powers were habitually feeble before the pneumonia occurred. One more remark is proper under this head, viz: that we have been more slow to allow solid food, to those who were recovering early, apparently from the influence of treatment, than to those in whom the disease had run through its natural course. Thus the shortest cases are the least liable to suspicion on this score.

Whether any other mode of determining the period of convalescence might be adopted is not now the question. It does not seem to me that any is more fair; but some further remarks and calculations in reference to this point will follow presently. The reader will perceive that we must adopt the mode which M. Louis has followed, in order to compare our cases with his.

In the calculations which follow, reference will be had in the first instance to all the cases; but there are three to which exceptions may be made. One is IX. the

second XIII. and the third XXV. IX. entered the hospital on the 15th day of the disease, and was convalescent on the 26th day; XXV. entered on the 14th day, and was convalescent on the 24th day; in both these the chance for treatment was so bad, and the disease so slow in advancing to convalescence, that it may be doubted whether they ought to be included. XIII. on the other hand, was already in the hospital when pneumonitis occurred, and was convalescent on the 4th day. The right of this case to be admitted into this table may be disputed. That the reader may judge for himself, the case will be given in detail. Meanwhile it will be deducted in the calculations, as will be seen, so as to show the general results without it.

The great object of M. Louis's researches is to show the influence of bloodletting, according to the circumstances of each case, and to the extent to which it has been carried. This therefore will be the first point to which attention will be given.

In four cases, XIII. XVI. XXI. and XXII. venesection was first practised on the *first* day of the disease. In these cases convalescence took place on the 4th, 14th, 13th, and 13th days, respectively. The aggregate of these days is 44, and this divided by 4 gives the 11th as the average day of convalescence in these cases. If we deduct XIII. for the reasons already given, we have for the other three cases the $13\frac{1}{3}$ as the average day of convalescence. In these cases, except XIII., venesection

was performed four times in each; in XIII. once; giving an average of $3\frac{1}{4}$ times if the whole be taken, or if XIII. be omitted, an average of four times for the rest. The quantity of blood abstracted was 49 ounces on an average for each case; or omitting XIII. it was $61\frac{1}{3}$ ounces.

What has here been stated in detail will be given in the table next following, (Table II.) and the succeeding tables will give the results of a corresponding nature, as to those who were bled on other days. The heading of one table will answer for all.

TABLE II.—*Cases in which Venesection was practised on the first day of the disease.*

No. of case in Table I.	The number of Venesections	Ounces of blood taken.	Day of convalescence.
XIII.	1	12	4th
XVI.	4	74	14th
XXI.	4	60	13th
XXII.	4	50	13th
4 cases	$13 \div 4 = 3\frac{1}{4}$	$196 \div 4 = 49$	$44\text{th} \div 4 = 11\text{th}$
deduct XIII.	1	12	4th
3 cases	$12 \div 3 = 4$	$184 \div 3 = 61\frac{1}{3}$	$40 \div 3 = 13\frac{1}{3}$

TABLE III.—*Venesection on second day.*

III.	2	28	13th
VII.	1	16	9th
VIII.	2	22	13th
X.	1	16*	14th
XI.	1	34	13th
XII.	4	80*	15th
XVIII.	2	37	8th
XIX.	3	42	12th
XXIX.	2	44	10th
9 cases	$18 \div 9 = 2$	$319 \div 9 = 35\frac{4}{9}$	$107 \div 9 = 11\frac{8}{9}$

* The asterisk affixed to the numbers in this and the other similar tables, is to indicate that the quantity was not exactly ascertained.

TABLE IV.—*Venesection on third day.*

I.	1	13	11th
XX.	3	52	11th
XXXII.	2	30*	16th
3 cases	$6 \div 3 = 2$	$95 \div 3 = 31\frac{2}{3}$	$38 \div 3 = 12\frac{2}{3}$

TABLE V.—*Venesection on fourth day.*

II.	1	7	8th
V.	1	16	9th
XXIII.	3 & leeches	50*	17th
XXX.	1	16*	13th
XXXI.	1	8	12th
XXXIII.	1	16	14th
6 cases	$8 \div 6 = 1\frac{1}{3}$	$113 \div 6 = 18\frac{5}{6}$	$73 \div 6 = 12\frac{1}{6}$

TABLE VI.—*Venesection on fifth day.*

XIV.	1	12	15th
XXVII.	2	28	10th
2 cases	$3 \div 2 = 1\frac{1}{2}$	$40 \div 2 = 20$	$25 \div 2 = 12\frac{1}{2}$

TABLE VII.—*Venesection on sixth, eighth, ninth, fourteenth, and nineteenth days; one case on each day.*

Day of Venesection				
6th	XVII.	1	22	13th
8th	XXVIII.	1	20	13th
9th	IV.	2	27	15th
14th	XXV.	2	30	24th
19th	IX.	2 & leeches	26	26th

If now we take those bled for the first time on the 1st, 2d, and 3d days together, it will be seen that there were 16 cases, and that the average period of convalescence was on the $11\frac{1}{3}$, or in decimals 11.81 day. But omitting case XIII. the result of the 15 cases will be that convalescence occurred on an average on the $12\frac{1}{3}$ or 12.33 day.

If we take those bled for the first time on the 4th day or before, as M. Louis has done, the result will be that in the 22 cases convalescence took place on an average on the $11\frac{10}{11}$, or 11.90 days. And again omitting XIII. we have as the answer the $12\frac{6}{11}$, or 12.28 day.

As opposed to the foregoing we may take all those bled for the first time after the 4th day, and we have 7 cases in which convalescence took place on an average on the $16\frac{4}{7}$, or 16.57 day. But, omitting cases IX. and XXV. we have 5 cases, viz.: those bled on the 5th, 6th, 8th, and 9th days, in which the average day of convalescence was the $13\frac{1}{5}$, or 13.20.

The whole number of those who were bled, was 29; and the average day of convalescence was $13\frac{1}{29}$, or 13.03. Or, omitting the three exceptionable cases, for the other 26 cases, it was the $12\frac{12}{26}$, or 12.46 day.

There remain 5 cases, in which bloodletting was not employed, except only six leeches in one of them. In these the period of convalescence was on the $14\frac{3}{5}$, or 14.60 day.

We thus see that, so far as the few cases I have furnished go to decide the question, we have shortened the period from the commencement of pneumonitis to the period of convalescence, (by bleeding on the 1st day,) from 14.60 days to 11 days. That is, we have diminished the period by about one quarter. If it be said that other remedies were employed, the answer is that other remedies were employed in all the cases. Next, if we

take the least favorable view of the effects of bloodletting, we have diminished the period about one tenth.

But this would not be representing the subject in a light sufficiently favorable to the cause of our remedy ; for, in truth, the cases, in which bloodletting was not employed, were much less severe than the others, taking an average on each side. So that the advantage, derived from bloodletting in our practice, is greater than that derived from the same treatment in the hands of M. Louis. It may be suspected that this difference is to be attributed to the other treatment employed by us. When all our statements have been made, this opinion will not appear very tenable. The average period for all our 34 patients taken together was $13\frac{9}{34}$, or 13.26. This is much less than for the cases reported by M. Louis. For this great difference I think the most probable explanation is that our hospital is much smaller than that of la Pitié ; that the comfort of the patients is provided for in every respect better than in the larger European hospitals ; and that, especially, there is always preserved in our hospital a higher temperature than in the Paris hospitals. If there be exceptions to this remark among the hospitals in Paris, la Pitié is not one of them, unless I have been misinformed.

It is very certain that our patients have an early convalescence, in proportion as they enter the hospital early after the commencement of their disease. In regard to the 34 cases in table I. this will appear by comparing the

dates in the second and third columns of that table with the period of convalescence. It will there appear that the patients entered from the 1st to the 15th day of the disease; the following table will exhibit the results.

TABLE VIII.

2 entered on the 1st day of disease, average convalescence $8\frac{1}{2}$ day.							
9	.	.	2d	.	.	.	$11\frac{2}{11}$
2	.	.	3d	.	.	.	13
7	.	.	4th	.	.	.	12
3	.	.	5th	.	.	.	$18\frac{1}{3}$
4	.	.	6th	.	.	.	$12\frac{1}{4}$
1	.	.	7th	.	.	.	16
4	.	.	8th	.	.	.	$14\frac{1}{4}$
1	.	.	14th	.	.	.	24
1	.	.	15th	.	.	.	26

As however our numbers are small we shall have a better and fairer view of the subject by the following statement.

Twenty entered on the 1st to the 4th day inclusive, and the average period for these was $11\frac{1}{2}$ days. In these is included XIII. a woman who was already in the hospital when seized with pneumonitis, and is represented therefore as having entered on the first day. Excluding her case as doubtful, the average period of the other 19 cases is $11\frac{17}{19}$, or nearly 12 days.

Twelve entered from the 5th to 8th day inclusive, and the average period for these was $14\frac{3}{4}$ days. Here is a difference (between 12 days and $14\frac{3}{4}$) of a little less than a fifth of the larger number.

Two entered, one on the 14th, and one on the 15th day of disease, and their average period was 25 days.

It will be seen presently that no other circumstance

exercised so great an influence on the period of convalescence as this. So that it would seem to be of less importance, whether our patients were bled or not, than whether they entered the hospital early or late. This is a result, which would not probably have been anticipated even by men of experience.

I shall now examine the other circumstances in regard to our patients in the order of the columns in Table I.

The following table gives the ages of the patients, whose cases are under our consideration, with the periods of convalescence; an average of that period being given, where two or more were of the same age.

TABLE IX.

Cases.	Age.	Convalescence.	
		Aggregate period in days.	Average period in days.
1	16 years.	18	18
1	17	9	9
1	18	13	13
1	19	14	14
3	20	$36 \div 3 =$	12
2	21	$22 \div 2 =$	11
3	22	$54 \div 3 =$	18
2	23	$31 \div 2 =$	$15\frac{1}{2}$
1	24	12	12
1	25	15	15
1	26	15	15
3	28	$42 \div 3 =$	14
1	29	13	13
3	30	$32 \div 3 =$	$10\frac{2}{3}$
1	31	12	12
2	33	$20 \div 2 =$	10
2	35	$38 \div 2 =$	19
1	36	4	4
1	37	14	14
1	42	17	17
1	45	8	8
1	47	15	15

If now we take the sum of the aggregate days of all these, from 16 to 20 years of age inclusive, we find it to be 90, and the cases are 7; so that we have $90 \div 7 = 12\frac{6}{7}$; and we may say, therefore, that on an average these patients were convalescent on the 13th nearly. In like manner those from 21 to 30 are found to have been convalescent on an average on the $13\frac{5}{7}$, or nearly the 14th day. But in these cases is included IX., one of the cases which may be deemed exceptionable. If this be deducted, $13\frac{1}{8}$, or, we may say, the 13th was the day of convalescence on an average. In the remaining cases the patients were from 31 to 47 years of age inclusive, and the average day for these is $12\frac{8}{10}$. But in these cases XIII. and XXV. are included. If these be deducted, the average day will be $12\frac{1}{2}$. Thus it happens, that those in the last division arrived the earliest at convalescence, and those in the middle division the latest; showing that in this small number of cases age had not a controlling influence.

The number of cases here considered is so small that we cannot make any important inferences from them alone. As to the influence of age they give results, different from those furnished from the more numerous cases of M. Louis. Perhaps an exact investigation of the circumstances would afford an explanation of the differences. As the period of the disease, at which our patients have entered, has been shown to have a great influence, I have looked to that source for an explanation. I do not find it however. It appears that those

of the first division entered on an average on $4\frac{1}{4}$ day ; those of the second on the $4\frac{9}{17}$; and those of the last in the $5\frac{1}{2}$. Thus in this respect, as well as in respect to age, the first division was under the most favorable, and the last under the least favorable circumstances.

In regard to sex, 25 of the cases were males, and their period of convalescence was on the $13\frac{4}{5}$ day ; 9 were females, and the similar period with them was the $13\frac{5}{9}$. Thus sex does not appear to have influenced the result.

In 18 cases the right side was affected ; in 12 the left side ; in the remaining 4 there was reason to suppose that both sides were affected.

It will appear in 7th column of table I. that our patients were not, all of them, in perfect health at the time, when they were attacked by pneumonitis. An examination of the table affords the following results.

Eleven patients were in good health, or are believed to have been so, at the attack, and in them the day of convalescence was on an average the $13\frac{10}{11}$.

Fourteen patients had acute catarrh, or catarrhal cough, in some of them the epidemic catarrh, (influenza). In these the day of convalescence was on an average the $12\frac{3}{4}$.

Nine of them had other and various complaints, and in these the day of convalescence was on an average the $13\frac{7}{9}$.

In the first of these divisions was case IX. of

which the duration was 26 days. In the last was case XXV., of which the duration was 24 days. While in the second was the other exceptionable case, XIII., of which the duration was only 4 days. If these cases be deducted from the several divisions, to which they belong, it will be found that the average duration for each, in their order, will be respectively $12\frac{7}{10}$, $12\frac{11}{13}$, and $12\frac{1}{2}$. The difference is so small, that we may infer, as safely as can be done in so small a number of cases, that the previous ill health in our patients was not of a kind to produce any important influence on the result.

In 29 cases vesication was employed about the chest, and on an average twice for each patient; the average period of convalescence in these was the 13th day, or a little later. In 5 cases vesication was not employed, and in them the average period of convalescence was on the $12\frac{2}{5}$ day. This result seems to confirm that of M. Louis. But I must believe that our results may be explained without admitting that vesication was injurious; and it might be rendered probable that it was useful. I may venture to say with confidence, from the universal practice among us, that vesication was omitted in the five cases, because neither pain, nor dyspnœa were urgent symptoms. If we look under the column of *Remarks* at these five cases, we find, in respect to XIX. that the pain and dyspnœa at one time, and the pain at another, were promptly relieved by venesection; and, in respect to the other cases, that they were not severe.

The inquiries in respect to the internal remedies will be more complicated, than those, which relate to the preceding columns. Some prefatory remarks will facilitate these inquiries.

It has been a common practice with us, and I believe in most parts of our country, to employ emetics and cathartics freely, as well as bloodletting, in the treatment of inflammatory diseases. They have been thought useful as modes of evacuation, and are tried by many in the first instance with the intention to avoid bloodletting, if possible. Besides, emetics, and especially antimonial emetics, have seemed to exercise an influence in abating inflammation, independent of the amount of the evacuations they have induced. This influence will probably be admitted by those of the present day, who employ antimonials for the same purpose. It will be found that emetics have been used in many of our cases, and cathartics in almost every one. But we have not the means of deciding their influence on the disease ; as other remedies have been used simultaneously, or very soon after them. While, therefore, I am disposed to think that they have often been beneficially employed ; yet as each case has had a chance of like benefit, or nearly so, they may be put out of the question in our endeavors to estimate the effects of other remedies. Further, it is more difficult to decide on their influence, because the other remedies so often act in some measure as emetics, or cathartics, or both.

Opium also has been employed in almost every case, more or less freely, as is the usual practice in our country. It has been employed, in combination with other remedies, to restrain their operation on the stomach and bowels within due bounds ; and, when the quantity thus given has not been sufficient to prevent excessive or painful coughing, more opium has been given for this last purpose. The use of it may be carried too far, no doubt ; and we think it is so, when the tongue is rendered dry ; when the pulse becomes more full, swelling under the finger ; or when it acts powerfully as a soporific, the respiration becoming heavy, and the cough being suppressed almost entirely. But, generally, it is thought by us to contribute to the comfort of the patient, without inducing any serious inconvenience. Usually it will be found that from half a grain to two grains are given in a day ; and especially when mercurials are employed. With colchicum, and with tartarized antimony, (tartar emetic) much less is usually given, and sometimes none at all. We cannot however attempt to show how far it has been beneficial, or not, upon the principles of the numerical method.

The three remedies, which are to be brought into comparison are mercurials, colchicum, (colchicum autumnale,) and tartarized antimony. These have been supposed to be useful in inflammatory diseases generally. It would be inconsistent with the plan of the present work to state the theoretical principles, on which their use has

been supported. The manner of exhibiting them, and the extent, to which they have commonly been employed, may now be most conveniently stated.

First, mercury. The preparation of this article, which is almost exclusively employed by us in acute diseases, is calomel, (*hydrargyri submuriatis*.) This is denoted in Table I. by H. S. It often enters into the composition of the cathartic administered in the commencement of the treatment. But, when mentioned in the table, it may be understood that it is employed in small doses, from one to four grains. In these doses it is given from once to four times a day. But it is no longer administered, or only once a day in a small dose, after the mouth is found to be affected by it in a sensible degree. In our hospital it is very rarely used, by design, to such an extent as to induce any marked, or free salivation; but occasionally, it must be allowed, such an effect takes place, and in some rare instances the salivation and soreness of mouth become very inconvenient. Such instances, however, are very rare in acute diseases.

With us the submuriate of mercury is very rarely administered alone. It has sometimes been given in combination with colchicum, or alternately with colchicum, on the same day, some opium being added. But it is more commonly given in combination with opium and tartarized antimony.

Such a combination is found in our *pillula hydrargyri submuriatis composita*, denoted in the table by *Pil. H. S. C.* This pill is prepared as follows.

℞. Antimon. Tartar.	gr. i.
Pulv. Opii.	gr. ii.
Hydr. Submur.	gr. x.
Mucilag. G. Arab.	Q.S.
M. f. pil. No. VI.	

Of these pills from one to eight are given in a day, most commonly four, one at a time, in the cases of pneumonitis, in which it is prescribed. The same articles are sometimes given in different proportions; but then a special recipe is furnished, and the article does not bear the name above given. All this is determined by the estimate, in the mind of the physician, of the constitution of the patient, and the violence of the disease. When it is evident that the disease is subsiding kindly, while the mouth is not yet sore, the use of the mercurial is omitted. Likewise, the medicine is abandoned, when it is found to occasion peculiar inconvenience to the patient.

Second, colchicum. Sometimes the root, sometimes the seeds of this article have been used by us in the manner and upon the principles laid down by Mr. Haden, of London, in his treatise on this article of the materia medica. That is, it is given in aid, or as a substitute for bloodletting; and it is given in such doses as to induce nausea at least, and usually vomiting and purging; and then continued in such doses, as can be borne by the patient without much inconvenience. For this purpose, from six to eight grains of the root, and commonly three of the seeds, are given at a dose; and at

first a dose is given once in six hours. The operative effects are not seen, in many cases, till six or eight doses have been given. Then they are often quite violent, copious sweating, as well as vomiting and purging, taking place; the pulse at the same time diminishing in hardness, force and frequency, and the muscular strength being greatly prostrated. The appearance of the amendment in such cases is sometimes very striking; equal to that which follows the most profuse bleeding; while the permanent reduction of strength has been thought to be less than that from large bleedings. Sometimes the relief seems to ensue without such powerful, temporary influence. But on the other hand the sufferings of the patient are often great, when this medicine acts powerfully; though I have never seen an instance, in which it seemed to increase the risk of life. In some cases, however, the patient seems quite unable to bear the continued use of this article for more than a single day. When colchicum and calomel have been used in the same case, it has usually been in succession; one being omitted, when the other has been prescribed.

Third, of tartarized antimony. It is useless to state how long antimonials have been employed in febrile and inflammatory diseases. The use of them, however, in large doses, frequently repeated, without a view to their operative effects, and with a design to avoid these in a great measure, is, I believe, comparatively of modern date. Such a use of them was first made known to me

in the work of Odier, of Geneva, on practical medicine,¹ which I read more than twenty years ago. I employed it, according to his doctrine, principally in typhus fever, when it assumed an inflammatory character. In later years its use has been extended among us to pneumonitis and other inflammatory diseases, in consequence of the recommendations of the Italian and French physicians. At our hospital we have most commonly followed the mode of Odier, which is the following:—A solution of the tartarized antimony is made of such strength that every fluid drachm may contain a quarter of a grain of the mineral. A dose of this solution is given every two hours. At first, half a drachm or a drachm is administered, but each succeeding dose is increased by half a drachm or a drachm, until nausea, vomiting, or purging is induced. As soon as either of these occur in an inconvenient degree, the dose is reduced to such a quantity as the patient can conveniently bear; or the medicine is suspended, till the operative effects have ceased, and then recommenced in a smaller dose. It will thus be perceived that the sensible effects of this medicine and of the colchicum are very similar, and so are the appearances of temporary relief. These last, however, have not been so striking to me in the case of the antimony, as in that of the colchicum. There are also patients, who cannot bear the continuance of the antimony, without

¹ Manuel de Médecine—pratique &c. par Louis Odier; à Paris et à Genève, 1811.

very distressing effects. On the other hand some patients endure it with very little, if any, immediate inconvenience, for three to six days, in very large doses. A case will be given in detail, in which the patient took nearly six grains every two hours. Two grains every two hours may be taken by many patients with very little sensible effect for several days in succession. At last, however, some patients feel a horror of the article, without being able to point out what evils they experience from it. When not well diluted, (an ounce of water should be added to each drachm of the solution, when administered) and in one case when diluted, I have known it produce redness, soreness, and even pustules, in the fauces. The powers of life are much depressed under its long continued use, and it has been suspected of contributing to a fatal issue of the disease, for which it has been given. It has been suspected too of acting on the stomach and bowels in a manner analogous to that, in which it acts on the skin and fauces. The only case, in which I feared that it might have contributed to a fatal issue, is the one which will be given in detail.

I will give the results, which followed the use of the three remedies above mentioned, as to the duration of the disease, taking the same criterion for the days of convalescence as before, and giving the average period for each group. It will be borne in mind that those who took mercurials always used small doses of antimony as well as of opium at the same time; but they are not here

noted as taking antimony also, unless where this was used freely in the manner stated above.

I. In 16 cases mercury was given, and a sore mouth produced; in these the day of convalescence was on an average $13\frac{5}{8}$, or, in decimals, 13.62.

From these we may deduct three, viz. VI. and XV. who were not bled, and IX. who was bled on the 19th day; and in the remaining thirteen cases, who were all bled as early as the 6th day, the day of convalescence was on an average the $12\frac{4}{13}$, or, in decimals, 12.30.

Of these sixteen cases, 12 also took colchicum to some extent, and in them the day of convalescence was on an average $13\frac{3}{4}$, or, 13.75; omit IX., and of the remaining 12 the days the $12\frac{7}{9}$, or 12.77.

One of these 16 cases took both colchicum and antimony in full doses, and was convalescent on the 13th day.

II. In 8 cases mercury was given, but the mouth was not made sore. In them the day of convalescence was on an average the $11\frac{1}{2}$, or 11.50.

In these 8 cases is included XIII., an exceptionable case. If this be deducted, we have 7 cases, in which the day of convalescence was on an average the $12\frac{4}{7}$, or, 12.57.

Of these 8 cases, 3 took colchicum also, and the day of convalescence was on an average the $12\frac{2}{3}$, or 12.66.

Of the same 8 cases, 2 took antimony also, and the day of convalescence was on an average the 15th.

III. In 17 cases colchicum was given and in these the day of convalescence was on an average the $14\frac{2}{17}$, or 14.11.

In these are included IX. and XXV.; and, if these be deducted, we have 15 cases, in which the day of convalescence was on an average the $12\frac{3}{5}$, or 12.66.

Of these 17 there were two only, who took colchicum without any mercury, or any antimony. These were XXV., convalescent on the 24th day, one of the three exceptionable cases; and XXIX., who was convalescent on the 10th day.

IV. In 8 cases tartarized antimony was given without mercury and without colchicum; in these the day of convalescence was on an average the $13\frac{3}{8}$, or 13.37.

Of these 8 cases, 2, XXVI. and XXXIV., were not bled. Their ages were 37 and 47, average 42. They entered the hospital, one on the 6th and one on the 8th day, of disease. In them the day of convalescence was on an average the $14\frac{1}{2}$, or 14.50.

Of the other 6 cases among the 8, the mean age was $26\frac{1}{3}$ years. They entered the hospital from the 2d to the 8th day of the disease; on an average on the 6th day. In these 6 cases the day of convalescence was on an average the 13th.

In reviewing these cases it will be manifest that the difference, as to the period of convalescence, is so trifling, that it may be regarded as nothing, whether we employ mercurials with, or without colchicum, or antimony, after

bloodletting. Of the effect of colchicum alone we have not the materials for forming an opinion.

It does not, necessarily, follow that these medicines had not any effect, or not any good effect, in our cases. For myself, I am ready to say, that under the responsibility which a physician owes to his patient, I should not, at present, feel at liberty to omit all the articles above-mentioned in the treatment of a case of pneumonitis. Yet, on the other hand, I should hold it unwise to persevere in the use of them in any instance, in which they should occasion peculiar inconvenience, or suffering to the patient.

The column in Table I. respecting the days of convalescence, has necessarily been referred to in describing the other columns, and needs not any separate remarks.

The next column respects relapses. It will be seen that the instances of relapse were very few ; and they all are accounted for by obvious errors. The reality of the convalescence after relapse on the days marked in this column may be confidently relied on.

The next, and last column but one, contains the date of the discharge of each patient from the hospital. It will be seen by the reader, who examines and compares with accuracy, that the period between the convalescence and the discharge differs very greatly in the different cases. Some shrewd man may hence suspect that the convalescence was not fairly, or exactly stated. But an examination of particulars would soon dissipate suspi-

cions of this sort. Various causes influence the discharge of a patient. One pays his board at the hospital, and on that account leaves it as soon as possible. Another is on a *free bed*, or is supported without expense to himself, and is anxious to remain as long as possible. The readiness with which men recover from acute disease, varies from constitutional differences; also from habitual care in one, and habitual carelessness in another. Accidental diseases of various kinds attack some patients during convalescence. Inclemencies of weather often prevent the discharge of a patient for several days after he is ready to go away. In these, and probably other ways, a great difference will be occasioned in the period between convalescence and a discharge from a hospital.

The last column, containing remarks, must occupy more of our attention; and it is hoped that it will repay our labor.

In the preceding work M. Louis has shown us what was the effect of venesection upon the particular symptoms of pneumonitis. The statements, which he has made under this head, are very valuable. They serve to prove that bleeding does more than shorten the disease, that it also mitigates its severity. The same inquiry is very interesting as regards every remedy employed. Indeed the inquiries of reasonable physicians in all ages, respecting the remedies they have tried, have been, 1st, whether they contributed to the safety of the patient;

2d, whether they shortened his disease ; and, 3d, whether they lessened his sufferings. In the last is included a comparison of the inconveniences produced by the remedy and of the relief, which it has given, to pain, or suffering.

I regret that I cannot answer all these inquiries fully and explicitly as to the cases, of which I have made an analysis. I cannot, because the records are not always sufficiently full and precise. The failure in these respects is found most frequently, when the patient has been obviously better, or relieved from dangerous symptoms. The seasons, when pneumonitis has been most prevalent, have been those when the influenza has been epidemic, as has before been stated. In these seasons our hospital has usually been uncommonly full, and at the same time the officers and nurses have been taken off by the epidemic. Hence our attention to the record of cases, which are doing well, has been diminished, that we might be more exact as to those, whose cases were doubtful. In addition to this, where different remedies are employed on the same day, such as venesection and vesication, it is not possible to distinguish between their effects. Having premised these remarks, I shall state what may be gathered from Table I., in reference to the change in symptoms after the use of venesection ; recalling to the reader's mind that blisters have followed bleeding, on the same day, in most instances.

The following table exhibits the most distinct view of the changes in the symptoms, following venesection, which I

have been able to obtain. The numbers in the first column, headed cases, refer to the numbers of the cases in Table I. By referring to that table, the reader may satisfy himself, what other remedies were in use on the days on which the bleeding took place.

TABLE X.

Case.		Day of disease.	Pulse less.	Pain less.	Sputa less colored.
XIII.	1st v. s. on the	1st	1st day	1st day	1st day ¹
III.	{ 1st " "	2d		1st "	
	{ 2d " "	4th	1st "		
VII.	1st " "	2d	1st "	1st "	1st "
XI.	1st " "	2d	1st "	1st "	2d "
XVIII.	1st " "	2d		1st "	2d "
XIX.	1st " "	2d	1st "	1st "	
XXIX.	1st " "	2d	1st "		2d "
I.	1st " "	3d	1st "	1st "	2d "
XII.	2d " "	3d	2d "		
XX.	{ 1st " "	3d		1st "	1st "
	{ 2d " "	4th	2d "		
XXI.	3d " "	3d	3d "		6th "
II.	1st " "	4th	1st "	1st "	1st "
VIII.	2d " "	4th		1st "	1st "
XVI.	2d " "	4th	1st "	1st "	
XXIII.	1st " "	4th	1st "	1st "	
XXXI.	1st " "	4th	3d "		
XXXIII.	1st " "	4th	2d "		
XIV.	1st " "	5th			2d "
XXII.	3d " "	5th	3d "	1st "	
XVII.	1st " "	6th	1st "	1st "	3d "
XXVII.	2d " "	6th	1st "		1st "
XXVIII.	1st " "	8th	2d "		
IV.	{ 1st " "	9th	1st "		
	{ 2d " "	10th	2d "		1st "
XXIV.	1st " "	14th	3d "	1st "	2d "
IX.	1st " "	19th	2d "		2d "

¹ In each of the last three columns the days are counted from the v. s. Thus in case XIII. the pulse was less frequent the 1st day after v. s.

In most of the above cases the effects of the first venesection only are stated ; in three cases the effects of the second are also stated ; while in six cases those of the first, or two first venesections are not stated, but those only of the second or third. In these last six cases the prior bleedings had not been followed by any change in the pulse, pain, or sputa ; or the changes had not been distinctly noted, if they occurred.

It will be remarked, by looking at the third column, that the cases are arranged in this table in the order of the days of the disease, on which the venesection was performed. On looking down the fourth column, it will be seen that, on an average, the pulse was lessened in frequency sooner in those who were bled early, than in those who were bled late. This differs from the results obtained by M. Louis. He will not fail to observe that, besides other remedies, vesication was usually made within twenty-four hours after the bleeding. Future observations, which will not be made with more fidelity by any one, than by M. Louis, may reconcile these discrepancies.

In the fifth column the pain is marked, as diminishing on the first day after v. s. when it is noted at all. The explanation is that the relief of the pain is not clearly connected with the bleeding in our cases, unless it occurs on the first day ; and, generally, when said to occur on that day, it took place at once, or in a few hours after the bleeding.

The last column has many blanks, which might have

been filled, if our records had been more precise. But in many cases the changes in the sputa are not mentioned, daily in our records. Considering the different periods at which v. s. was performed, and that in all the cases, except two, the sputa mended on the first or second day afterward, it seems that the bleeding had an influence on them. It may be supposed however that in the cases, where there are blanks in this last column, there was not any change in the sputa soon after the bleedings, and therefore no note was made. It might be so in some, but not in most of these cases, as I judge from examining the records. The results, as regards the sputa, do not agree, more than those respecting the pulse, with those of M. Louis.

The inference, however, to which we are brought by this table, as well as by the observations on the same points in M. Louis's researches, is that the relief to some of the most peculiar symptoms of pneumonitis following bloodletting was unequivocal; and that it was probably much greater than we should be induced to suppose by attending only to the shortening of the disease by the same remedy. Or, if it be said that the whole benefit is not to be attributed to the loss of blood, then the remark will hold good in respect to the remedial measures generally. How far one or another contributed to this relief may be judged, in some measure, from comparisons heretofore stated.

I regret that our records do not enable me to state the

effects of remedies in respect to dyspnœa. In some instances the relief of this symptom after v. s. and blisters is distinctly noted. In others, expressions are used, from which I have not any doubt that it occurred at once, or on the day following the use of these remedies. But as these expressions are not explicit, I have not ventured to ground any exact statements upon them. The effect produced on the eye and feelings of the physician by difficulty of breathing is such, that he would never speak of the patient as having a better aspect, or as appearing decidedly relieved, when that difficulty had not diminished. When these expressions are used, therefore, it is tolerably certain that the dyspnœa is less.

It is a subject of regret that the physical signs of disease in the cases, which have been under examination, could not be reported. This is not because they were not noted ; for in most of our cases they were so. It is because neither my colleagues, nor myself have felt so sure of our accuracy in distinguishing these signs, until within the last two or three years, that we should choose to place any reliance on our notes. We were learners, and made notes with a view to find out when we were right and when we were wrong. In fatal cases we compared our notes with the results. The number of cases in these latter years was so small, that it did not seem advisable to introduce our record of the physical signs in these only.

I shall conclude this Appendix by stating in detail

three cases of pneumonitis. The first is given merely as a specimen of our cases; but it was selected particularly on account of the erysipelas, which occurred in the patient during convalescence. It is case XX. in Table I. The second is case XIII. in Table I. in which convalescence took place on the 4th day of the pneumonitis. It was preceded by influenza, and it was for this the patient had entered the hospital. As this is the only case in that table, in which the disease seems to have been arrested, almost at once, by bloodletting, and as its claim to be admitted as pneumonitis may be questioned, I give it in detail. The third is a fatal case not included in Table I. Some remarks will follow it.

FIRST CASE.

Pneumonitis, followed by erysipelas of the face during convalescence.

N. P. S. aged 33, a farmer from New Hampshire, entered March 29th, 1832. He states that he had the influenza early in winter, and since that has had a slight cough. His present illness commenced yesterday morning, when he had a chillness, which caused a shaking and chattering of his teeth, his skin being at the same time quite hot, accompanied by pains all over him, by cough, loss of appetite and nausea. He took at that time a dose of lobelia and red pepper, which produced vomiting and purging. After this he had a sense of relief as to the stomach, but his other symptoms continued.

Now, 6 o'clock P. M. he is in bed, lying on his right side, face deeply colored, eyes heavy and suffused, respiration hurried, without pain, has a frequent, short cough, attended mostly with a bloody, frothy expectoration. He complains chiefly of a universal soreness and tenderness over the surface, has not much pain except in the head, is chilly and hot by times, is disposed to sleep, but when roused becomes restless. Tongue coated and moist; p. 100, small and feeble; skin hot and rather dry; respiration attended with a slight mucous rattle in throat.

Pil. Hyd. subm. comp. One to night and one in the morning.

March 30th, morning. His night has been uneasy, without any sound sleep; his cough is not frequent, but occasions severe pain in right mammary region, expectoration as last evening; p. 84, soft, but of tolerable strength and volume; respiration not quick, nor full; expiration accompanied for the most part by a slight grunt, and frequently by a hacking cough, without expectoration; countenance a little flushed; skin not hot; considerable thirst; tongue rather pale, with a slight, white irregular coat; three or four dejections since entrance; urine scanty, color not noticed.

Mittatur sanguis ad $\frac{3}{4}$ xx.

Cerat Cantbarid. 7 inches by 6, on right breast.

Pil. Hyd. subm. comp. one every six hours.

At evening hot fomentations from knees to ankles for an hour.

March 31. A sense of relief followed v. s. Was easier during day and night ; slept, as he thinks, half the night, but when awake was troubled by pain in the chest ; was sensible of relief from the vesication. Countenance flushed ; respiration nearly as yesterday, but more distressed ; cough frequent ; expectoration rather more copious, less distinctly bloody, of a red, rusty color ; p. 120, and soft, when sitting up ; two dejections ; a chilly turn yesterday, A. M. and again in evening, not of long continuance.

Repeat v. s. till relieved, if distress in respiration recur in evening, v. s. ad 3 xii . Let him now have the following,

R. Sol. Magnes. Sulphat. 3 iv.^1 Repeat fomentations.

April 1. Twenty-four ounces of blood taken yesterday morning. Through the day more comfortable than on the preceding ; talked in sleep. Coughs a good deal, sometimes without expectoration ; expectoration four ounces of frothy, viscid mucus with a uniform mixture of blood, approaching a vermilion tint more than heretofore ; countenance less flushed, its expression less anxious ; p. 116 after getting up and down twice ; tongue moist, of good color, not much coated ; has appetite, with very little thirst ; three dejections.

By stethoscope ;—below right clavicle extending to mamary region, a crepito-mucous râle ; beneath right

¹ This quantity of the solution contains one ounce of the salt.

scapula crepitous râle ; lower down on back respiration vesicular. On percussion, flat in right infra-clavicular and mammary regions. On the left, in front, respiration loud, rough, sibilant ; natural on percussion.

If there be increase of pain, of distress in respiration, or of fever, v. s. as directed yesterday.

Cerat. Canthar. 7—6 below right scapula.

Omit Pil. Hyd. Sub. Comp.

R. Hyd. Subm.	gr. xv.
Pulv. Opii.	gr. i.
Antim. Tartar.	gr. i.
Muc. G. Arab.	Q.S.

M. f. pil. No. vi. Let him take one every six hours.

April 2. Reports that he is better and has been so through the day and night. P. 96, soft and indistinct ; expectoration more copious, but of the same character ; tongue the same ; four dejections ; no chills, nor flushing of the face.

By stethoscope ;—in anterior part of right thorax muco-crepitous râle ; in right scapular region ægophony.

Omit pills of yesterday. Pil. hyd. sub. comp. one night and morning. If violent symptoms return v. s. and pills of yesterday.

April 3. P. 96, fuller, harder ; skin hot, face flushed ; reports not so well ; day very comfortable ; a little sleep early in night, then restlessness ; some sleep latter part of night and this morning ; expectoration less bloody, but still crude and viscid ; lies most easily on right side,

where he has occasionally sharp, shooting pains; two dejections; some soreness of gums.

By stethoscope;—ægophony very distinct in right scapular region.

Omit pills. R. Pulv. Colchic, rad. ʒss.

Potass. et Sodæ Tartrat. ʒss.

M. Div. in Ch. No. V.

of these powders let him take one every six hours.

V. S. ad. ʒviii. vel x.

At bed-time. Pulv. Opii. gr. i.

April 4. P. 88. less full; skin soft; countenance less flushed; more easy; less groaning; reports more comfortable and has been so through day and night; expectoration as before, except two sputa of semi-opake mucus; three dejections without pain.

By stethoscope,—ægophony diminished in extent.

Powders of yesterday, one every eight hours. Same opiate at night as last night.

April 5. One dejection; p. 72, very soft; expectoration viscid mucus, less rusty and some of it not at all colored; cough more catarrhal in its character, shaking and distressing the head.

Omit the powders of the 3d.

R. Balsam. Copaibae. gtt. xx.

Syrup. Simp. ʒss. M. to be taken 3 times a day.

Opiate at night, if need.

April 6. P. 48. Expectoration entirely without color; four dejections with some griping; slept well with-

out opiate ; now, lying on left side and breathing easily ; appetite very good.

Continue balsam, &c.

April 7. P. as yesterday ; skin soft, moist, warm ; tongue cleaning ; one dejection ; blister on scapula still open and sore ; sleeps well without opiate ; has appetite, desires fresh pork and eggs.

Let him have milk porridge.

April 8. Milk porridge grateful and no trouble at stomach from it ; tongue never cleaner ; slept well through most of the afternoon and night ; p. 60 ; cough rather harder ; 3 or 4 sputa slightly colored ; room got cold in the night.

April 9. Out of bed and dressed ; looks better ; tongue clean ; p. 72 ; one dejection ; head dizzy and not easy ; vision not quite distinct ; cough less easy by his report, but sounds loose.

Omit medicine.

April 10. Symptoms in head increased with some pain ; much giddiness on rising from chair, or on sudden motion ; slept tolerably ; felt cold in night and the same the night before ; soreness about angles of jaw, across the nose and extending towards and below left eye, with erythematous redness there, but little swelling ; pulse, tongue and skin as well ; nasal mucus dry and hard ; no dejection.

R. Infus. Sennæ Comp. ʒii. to be taken now and repeated once in 3 hours till a free dejection. Apply to

inflamed parts on the face compresses wet in diluted Alcohol.

Pil Scill. Comp. at bed time.

April 11. P. 68, hard, full; erythema diminished on left side of face, extended on the right and there slight vesication; expectoration easier and sputa more distinct.

R. Sol. Antimon. Tart. $\mathfrak{z}\text{i}$. to be taken every two hours; if nausea, diminish the dose, Repeat pill at night.

April 12. T. rather more coated; pulse and skin well; erythema nearly gone from left side of nose, remains on right cheek, but abating; less sore on sides of neck; throat sore, but no redness, deglutition easy. The first dose of the solution occasioned nausea and vomiting; the second ($\mathfrak{z}\text{ss}$.) was well borne, and subsequently the full dose was equally well borne; five dejections small and loose.

Let him have the solution once in four hours, unless diarrhœa continue; if so, omit it.

For diet, bread and porridge.

April 13. Some pain yesterday above left eye and temple; p. 60; tongue nearly clean; slight epistaxis two or three times daily; expectoration, mucus, somewhat opake, with one or two streaks of blood; the source of this doubtful; one dejection yesterday; some giddiness when sitting up, none in bed. Very little hardness only remains in skin of the face.

Solution once in six hours.

April 14. Continues to mend; appetite increased; face nearly well; cough continues.

Increase food gradually.

April 15. Altogether well; yet is feeble, probably from lying in bed.

Let him be up and dressed.

Broth and bread.

April 16. Up and dressed; looks well; expectoration small, not morbid; no remains of erythema; walks about the ward.

Omit the solution.

From this day he was convalescent, and on April 25th was discharged well.

SECOND CASE.¹

Entered for influenza, was convalescent, and then seized with pneumonitis; from which convalescent on fourth day.

Dec. 10, 1831. A. B. female, aged 36. Cook in hospital. Has had a cold for a fortnight, but not much sick until the 8th. Since then, very unwell, i. e. headache, chills, pain all over, no appetite.

Now on the bed; pulse 84, neither full nor hard. Face and skin generally flushed, veins distended; skin rather warm, not dry. Tongue very slightly coated, not moist. Eyes suffused, eyes and nose flowing. Pain in head, eyes, and limbs; also at epigastrium passing to sides and bowels. Nausea, disgust of food. No dejection for two days. Took last evening the following:

¹ This is case XIII. in Table I.

R. Pulv. Ipecac et Opii.

Hydr. Submur. aa. gr. vi.

M. —————

This morning, the following :

R. Solut. Antimon. Tart. ℥i.

“ Magnesiae Sulphat. ℥iii.

M. Repeat p. r. n.

If pain in side become urgent, venesection and vesication.

11th. Vomited bile copiously many times, with much straining, but with much relief. Afterward repeated sulphate and had enema ; then followed eight dejections, copious. No pain in chest. Pain in limbs continues ; not in head, but this remains dull and stupid. Tongue as yesterday. Pulse 72. Respiration easy. Nasal catarrh, also some cough with expectoration of serous fluid and some porraceous matter.

R. Pil. Aloes Comp. to be taken at bed time if uncomfortable.

3 P.M. Comfortable.

12th. Day and night comfortable ; too weak to sit up ; soon tired by the attempt ; nearly free from pain. Cough not urgent. Three dejections ; did not take pill.

13th. Up and dressed ; feels better ; not strong ; bowels open.

14th. The same ; gains no strength ; annoyed most by pain in limbs. Pediluvium at night.

15th. Better. Reports well.

16th. In bed; not so well. Comfortable till 4 A.M.; since then, pain in left shoulder and left side, quite sharp at first; now not severe, except on inspiration. Pulse 120, small and rather hard. Pain was preceded by chill. In the night by accident a blanket was removed from the bed. Took more food yesterday, but without any evil. Now flushed and rather warm. One dejection sufficient.

Venesection to 12 ounces. Blister 6 inches by 4 to side. If not evidently relieved at bed time, take Pil. Hydr. Submur. comp.

17th. Slightly faint from bleeding. Before evening pain much mitigated. Pulse 90, small, rather hard. Countenance flushed, but less so. Tongue has more formal white coat. Not much cough; sputa transparent mucus; some of reddish, more of yellowish color; soreness under sternum and in left thorax; more in acromial region. Wakeful in night with sense of faintness; no pain nor cough; sighs as if from feeling of faintness. No dejection. Blood buffed and slightly cupped.

R. Solut. Magnesiae Sulphat. ʒiii. now, and in five hours repeat if no dejection. Pill to be repeated at night. If recurrence of pain, vesicate.

18th. Better through day and night. Countenance much improved. Feels better. Pulse sufficiently strong and natural; tongue mends a little; pain and soreness nearly gone. Coughs very little; expectorates a little mucus, partly gray. Three dejections after one dose.

Omit pill unless symptoms return.

19th. Up and dressed. No pain in side ; same pain continues at bottom of sternum. Neither cough nor expectoration. Appears languid ; has appetite ; tongue not clean.

Diet. A little bread.

20th. Feels and looks convalescent ; has appetite. Increase food gradually.

21st. Doing well. Tongue clean. No dejection.

R. Solut. Magnesiæ Sulphat. ʒiii .

22d. Tongue more clean. Otherwise the same ; two dejections.

23d. Doing well ; strength returns slowly.

24th. Gains strength.

25th. Stronger ; better.

27th. Doing well.

28th. Appears and reports well. Discharged well.

THIRD CASE.

Pneumonitis, entered 12th day, fatal on 20th day.

January 28, 1834. Connelly, laborer. Male. Æt.

35. Large, full chest. Has had cough and dyspnœa for four years, for the most part without expectoration, and never sufficient to interrupt him in his occupation. On the 17th instant was engaged in shovelling mud from a scow into the water. This his usual employment. He was thus occupied for two hours, during which time the water was constantly splashing against him, and freezing upon his clothes. He was at this time in a profuse sweat,

and immediately sailed for an hour, standing at the helm. During the remainder of the day was chilly, had an augmentation of cough, with pain in right side of thorax, and much dyspnœa. At midnight was intensely hot; afterwards sweat profusely. Next day cough increased. Has been confined to bed since his attack; has had chills and heat daily, constant pain in chest on full inspiration, especially in right side; also, on coughing, pain in the head. Frequently distressed for breath. Has not been bled, but has taken an emetic. The sputa were white on the evening of the attack, on the following day red, &c.

3 P. M. Pulse 128, skin warm and moist, perspiration on face. Pain below right mamma on full inspiration. Cough frequent, for the most part without expectoration.

7 P. M. Pulse as before, full and hard. Skin extremely hot and dry, face flushed. Scarcely any sleep since attack.

Venesection p. r. n.

Cerat Cantharid. 6—5 over seat of pain.

R. Liq. Ant. Tart. 3i. every two hours.

29th. Sixteen ounces of blood taken with relief of pain and dyspnœa, not faint, blood not buffed. Coughed much in night, had but little sleep; yet he says his night was far better than the night previous. Sputa about two ounces adhesive mucus, rusty, distinctly bloody in some parts. Pulse 112. Tongue moist now, has been dry in sulcus, clean at edges, white on lobes.

On right back crepitous râle, finer below, gradually

becoming more coarse as you ascend, getting into mucous at the upper part ; occasionally bronchial respiration on the right back. In right breast occasional bronchial respiration, vesicular murmur and râles wanting or nearly so. Left back respiration noisy, vesicular. On percussion, flat over the whole of right back, least so at the top. Same as to the right breast, most flat at the lowest part.

One dejection in evening moderate, one in night copious, loose and of good color. Urine bright, high colored, without sediment.

Continue solution of antimony, increasing each dose by ʒi. unless nausea or vomiting, then lessen the dose, or if there be nausea or two dejections after a dose of less than ʒiii. add to each dose the following :

R. Tinct. Opii. gtt. v.

30th. Flushed P. M., but not hot. More tranquil day and night. Short naps terminating in starts. Not much cough in night. This morning better. Pulse 112, rather hard, not full. Skin temperate, rather moist than dry. Respirations 36. Urine sufficient, high colored, turbid, without lateritious sediment.

In front percussion nearly as good on the right as on the left breast. In right thorax, respiration substantially as yesterday ; bronchial sound more evident, especially on the back. On left back, lower half, crepitous râle, yet the respiration is more vesicular than on the right.

Two dejections, small.

Says he has no pain, but points to right breast, saying

there is something there "smothering him," preventing his cough.

Has taken 3xii. of Solution of Antimony at a dose without nausea.

Apply under the scapulæ, most under the right, the following :

Cerat. Cantharid. 6—8.

31st. Tranquil in the day ; slight flush on face. At times cool in day, more so in night : some good sleep in latter part of night. Countenance languid, some yellowness of skin, had the same yesterday. Cough less troublesome. Expectoration less viscid, more easy. Pulse 96, rather hard. Respirations 28. Tongue moist at edges, dry with scales over tip and middle. Still complains of right breast near mamma, more than of any other part.

On right back crepitous râle, more distinct in lower half, not heard above scapula. Also, bronchial respiration above scapula and below, less below. In right breast crepitous râle with bronchial respiration.

On percussion in front, when lying on back, more flat in both breasts than yesterday, especially in the right.

Takes 3xxiii of solution at a dose. Slight nausea and retching after one dose. Four loose dejections ; three of them rather copious.

Continue at this dose unless nausea.

Cerat. Cantharid. 6—4 on right breast.

Feb. 1st. Since 12 M. yesterday has vomited four or

five times, mostly a green fluid, the last light straw colored. Otherwise, day comfortable. Restless early in night. Some groaning in sleep. Two dejections, small and loose. Coughed less. Sputa very little, if at all colored; this morning without blood, getting to be opaque, puriform. Pulse 90. Respirations 24, more full. Skin temperate, a little moist, especially on face; sweat noticed in night. Urine a little dark, quite transparent and free from sediment. Tongue covered with scales, more continuous than yesterday: one small pustule on the tip, moist at sides.

Over right back, coarse crepitous râle every where above scapula; quite at apex, respiration bronchial. Left back, crepitous râle diminished, less extensive; where there is no râle, respiration puerile, coarse; the same (puerile and coarse) in left breast, except perhaps a sound of distant crepitous râle on expiration. In right breast, coarse crepitous râle: the respiration less vesicular than behind. On percussion, upper part of both breasts less flat than yesterday.

Omit Antimony.

Feb. 2d. More restless through day and night. Vomited often until night a green fluid, less than half a pint. Five small dejections. Cough rather lessened. Expectoration not bloody, but is mixed with the fluid vomited. Now respirations 40, labored, irregular. Pulse 120. Tongue more dry on sides, otherwise as yesterday. Countenance fatigued; much sweat on face.

Says his pain is in the right breast passing down across the abdomen ; feels "bound" in the abdomen ; complains on pressure at every part of abdomen, which is full, but not tense, resounding on percussion at superior parts, less so inferiorly, i. e. the right side on which he lies. Urine as yesterday. On percussion, resounds sufficiently well in both breasts, rather less on right. Râle not materially altered from yesterday. On the neck, head, face and breast, a copious eruption of very minute pustules, prominent, size of a pin's point, with an areola round each of a pale red color : the whole intermediate skin is reddened : some pustules about the groin also, very few elsewhere.

Cerat Cantharid. 9—7 over right thorax down on to the abdomen.

R. Tinct. Opii. gtt. xxx.

Aquæ ʒss. M. to be taken immediately ; if rendered more comfortable by this, repeat not oftener than once in six hours.

Enema now.

Feb. 3d. Generally distressed through the day, at times delirious. Countenance livid ; it was thus livid at 5 P. M. Had one dose of the opiate in morning without relief. At 5 P. M. had a second dose of 40 drops. After this, dyspnœa increased and the opiate was omitted. This distress lasted until after midnight, since then has been getting easier. Vesication took place in night. After enema two dejections, the first copious, light green.

Coughed very little. Sputa almost nothing, dried up, opaque mucus, frothy.

Now, pulse 120. Skin pleasantly warm, inclining to moisture. Complexion much more natural. Countenance less fatigued. Respiration 30, more full, less labored; yet, dilatation of alæ nasi. Eyes very heavy. Pustules of yesterday a little flattened, the areolæ increased in size. Teeth crusted and foul. Tongue rather less dry. Abdomen less full, easier; less tender on pressure. No delirium. Much sweating during day and early part of night.

Right breast, the râle is more coarse, rattling, mucous, with more or less vesicular respiration. Right back, respiration bronchial nearly to lower part of scapula; below this, crepitous râle getting finer as you descend. Lower half of left back, crepitous râle. During visit has expectorated three times. Has taken but little nutriment. Has much thirst.

May have barley water with sugar and lemon juice. ℥ss. to ʒi. not oftener than once an hour.

Enema. And repeat if no dejection.

If cough become distressing without easy expectoration, take ℥ss. of the following every hour till relieved.

R.	Syrup Tolu.	} aa ℥ss.
	Syrup Scill.	
	Muc. Acac.	ʒi.
M.	Tinct. Opii.	ʒi.

If increase of distress and dyspnœa, apply poultice to blister.

Feb. 4th. From 4 P. M. to 4 A. M. much dyspnœa, and cough without expectoration, accompanied with profuse sweat some of the time. At some periods, complexion livid. Two dejections after enema not small, loose, of good color. Took the mixture 3 times without relief. Not comforted by poultice. Acid in barley water not grateful. Has taken a pint of barley water. Expectoration almost nothing since 4 P. M. The effort produces nausea. Pulse 140, small and weak. Respirations 36, more full. Urine rather dark, perfectly clear. Tongue less swollen, dry, scales less thick.

In left breast respiration is coarse though vesicular. In right breast muco-crepitous râle, more expansion below than at the upper part. On the right back respiration as yesterday, except that the bronchial extends somewhat downward.

Eruption fading. Muscular strength less. Abdomen rather more full than yesterday; still tender; thinks he feels better there.

Cerat Cantharid. 8—6, on right breast.

Enema.

R. Syrup Senegæ 3i. to be taken every 2 or 3 hours, if expectoration is made more easy by it.

Feb. 5th. Respiration 48, labored. Pulse 140, rather hard, not easily compressed but smaller. Skin warm, moist. Face more pale, and in a full sweat. Vomited a little yesterday. Five dejections, one of them copious. No relief from Syrup. It produced nausea and was

therefore omitted. Cough frequent, not long continued. Very little expectoration, light colored, opaque mucus, frothy. Tongue not dry but pasty in the middle. Has not had any opiate. Vesicated without obvious relief. Abdomen not full nor tense, little or no pain there. Urine as before. Prone to sleep in short naps.

R. Tinct. Opii. ʒi.

Syrup Tolu. }
Muc. Acac. } aa ʒi.

M.

Take ʒii. every two hours until three doses are taken ; then once every four or five hours, unless too much influenced by the opiate.

6 P. M. Died.

Autopsy, Feb. 6, 1834. Fourteen hours after death. *Externally*, percussion flat over right thorax, laterally and posteriorly. Surface of abdomen and legs covered with numerous little white vesicles ; back livid, studded with innumerable small pustules as during life. Integuments of chest, both skin and muscles peculiarly moist ; a little serosity flowing from them on incision.

Abdomen, pharynx and œsophagus natural.

Stomach two or three times its natural volume ; intestinal surface covered with a great quantity of attached, though not very adhesive mucus : color generally of a grayish red, in some parts in dots, elsewhere more generally diffused. Near and about cardia and in great cul de sac little grayish points, large as mustard seed, having

their seat in the mucous membrane, and seem like collapsed vesicles on its internal surface, when detached: mucous membrane mamelonated, pale, not very marked about pylorus. Thickness and consistence natural. Strips three or four lines; greatest at great cul de sac, five or six on faces, and twelve to fifteen on small curvature. *Small intestines*, volume natural, containing a pultaceous sort of mucus, closely attached, but not adhesive, yellow; in upper two thirds mucous membrane pale; below, red in some patches, but not very finely injected. Black points in Peyer's glands very manifest; otherwise as usual. Several of Brunner's glands, with black points, and enlarged, in the last foot or two: mucous membrane natural in thickness and consistence, except in last foot or two, where it is a little thickened, and less consistent than natural; strips two or three lines at most.

Large intestine moderate size; mucous membrane generally pale; but in some parts of a pale red. In the first foot Brunner's glands enlarged, and some looked like pustules of a yellowish white color, round, acuminate; mucous membrane over them destroyed, letting escape at the orifice a yellow white mass, nearly as large as the head of a pin, and resembling effused lymph. Similar affection of a pustular form in last foot of intestine; mucous membrane lessened in consistence in many parts.

Liver large, of rather a pale aspect, granular; natural consistence.

Spleen natural in size, color, and consistence.

Kidneys, the external coat still adheres a little, i. e. a thin lamina remains after attempts to detach it ; substance very granular to eye when this lamina is detached : consistence good.

Thorax. Pericardium contains two or three ounces clear serosity. *Heart* distended with blood ; firm, fibrinous coagula in each side ; organ natural ; valves natural.

Right lung adherent every where, except anterior edge and portion of base ; adheres by recent false membranes easily broken ; lung large, heavy ; retained its full size when removed from cavity ; crepitates only at anterior edge of upper lobe, and in lower portion of lower lobe. Surface, generally, of a somewhat red hue. No polish upon pleura except in parts already noticed as free from adhesion ; upper lobe hepatized almost throughout, passing into third stage, and at one part near apex quite broken, of a grayish, dark red color superiorly, becoming more red below ; yielding pus on pressure at various points, especially superiorly ; granulations very distinct, like little white bodies, very numerous ; easily penetrated by finger. Lower lobe in same state, but less advanced and still containing some air. Mucous membrane of bronchia, of a deep dark red, and having a thickened aspect.

Left lung generally healthy : a few slight adhesions laterally. Pleura preserves its polish almost throughout : this lung lighter, smaller, paler than the right ; generally crepitating ; solid, in limited portion of lower lobe posteriorly, near vertebral region ; thought to be hepatized in this portion.

REMARKS ON THE THIRD CASE.

This case is published, because it is the only case in our records, in which pneumonitis on one side only, without any other disease, has proved fatal. In the other fatal cases the pneumonitis was double; or, while one lung only was affected, there was also pericarditis, or some other disease of magnitude. The intelligent reader will join me in the doubt, whether the foregoing can be said to be a pneumonitis of one side only.

It is true however that, while the right lung was greatly diseased, the left scarcely exhibited any, if any, marks of inflammation after death; so that the patient may be thought to have died of single pneumonitis. But, if our estimate of the physical signs during life was correct, there had existed inflammation of the left side also. I can hardly doubt our accuracy as to the physical signs, because they were observed separately and independently by my late son and myself. At the time when this case occurred I had reason to believe that I had at length acquired the art of distinguishing those signs with some exactness; and my son had recently returned from Paris, where he had studied the arts of auscultation and percussion, as it seemed to me, with great success. I should not have indulged in these remarks, were it not to support the pathological conclusion, which the results in our hospital would lead us to make, viz. that simple pneumonitis of one side, in adults, is very rarely, if ever, fatal among us.

It may be noted that this patient took an extraordinary quantity of tartarized antimony ; for one day his dose was five grains and three quarters every two hours, making nearly seventy grains. The medicine was stopped from an apprehension of evil, though none then appeared ; but afterwards he had copious and very urgent vomiting.

I well remember that we feared, at the time, that the medicine might have contributed to the fatal issue ; although, when it was administered, the case had a very formidable aspect, both lungs appeared to be diseased, and we thought the patient gained something under the use of it. May it be that the slighter disease in the left lung was removed by its influence, while that of the right lung resisted it ?

While the foregoing sheets have been going through the press, it has occurred to me that it would be satisfactory to many readers to know the proportion of fatal cases of pneumonitis in our hospital. I had neglected to take notes on this point while examining the case-books. The time would hardly permit me to go through all of these books. I therefore limited myself to those of the last four years ; beginning with December 3d, 1831, when a new volume commenced, and just when pneumonitis began to prevail in connexion with the influenza of that season ; and ending with the corresponding date

in this year, 1835. In this research I have noted all the cases of pneumonitis, except those which were manifestly secondary, such as occur in typhus, phthisis, &c. That is, I have not excluded cases merely because the patients were manifestly tuberculous ; nor have I excluded cases, as I did from Table I. merely because the dates of the commencement, of bleeding, of convalescence, &c. were not well ascertained. These considerations, the last especially, were of no consequence in our present inquiry. These explanations are made to show why the number is so much larger than Table I. would lead the reader to expect.

I find then, in the period referred to, that the whole number of cases is fifty-one ; and among these the recoveries have been forty-three, and the deaths have been eight. Autopsies have been made in all these cases except one. In no one of the cases, where autopsies have been made, has there been found to be a pneumonitis of one side only, except in that of Conelly, which is given above. The reader will see how much doubt there is whether that should be regarded as a single pneumonitis. In one of the fatal cases there was pericarditis, the diagnosis well marked before death, and much serous pus found in the pericardium after death. In another the diagnosis was even more satisfactory before death as to pericarditis, but this was the one in which no autopsy was made, the friends not consenting to it. In a third there had been cough for ten years, and this had been worse

for seven months before the pneumonitis ; there had also been signs of organic disease of the heart for a year or more. After death there were found emphysema of the lungs, and hypertrophy of the left ventricle of the heart.

Of the cases here enumerated, all were not severe ; but a large proportion of them were so, as will usually happen in hospitals ; in several of those not fatal it was obvious that the lungs on both sides were diseased ; and in one there was undoubtedly a pericarditis, from which the recovery was complete.

The inference to be drawn is, that pneumonitis, when single and uncomplicated, in an adult, not tuberculous, not having any grave disease previously, will very rarely prove fatal. No one will suspect, surely, that I mean to attribute this result to any peculiarly happy treatment in our hospital ; but I am aware that, under circumstances less favorable as respects temperature and common care, the result may not be so happy.

In conclusion, many readers may ask if it is thought that the researches, of which this volume contains the results, are to be considered as leading to any positive conclusions. Certainly not. M. Louis has done us great service in stating his own accurate observations. They must have great weight in the minds of reflecting men. We have added all the observations that we have of sufficient accuracy to be compared with his, which will be received for what they are worth. The whole are to be regarded as materials, to which others are solicited to

make additions from time to time ; that, at length, so many cases, impartially collected, may be brought together, as shall justify entire confidence in the inferences to be made from them. Ten hospitals, under the care of honest physicians, may settle the questions discussed in this work within five years, so that our posterity will not for ages be able to make any material correction in the answers. Seasons and epidemics will vary no doubt ; but the general laws will be found the same, and little else would remain for future ages than to settle the allowance to be made for disturbing forces.

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