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THE  
BRITISH AMERICAN JOURNAL

OF

MEDICAL AND PHYSICAL SCIENCE.

EDITED BY

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VOL. III.

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MONTREAL:

PRINTED AND PUBLISHED BY JOHN C. BECKET, 211 $\frac{1}{2}$ , SAINT PAUL STREET.

1847-1848.



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THE  
**BRITISH AMERICAN JOURNAL**  
OF  
**MEDICAL AND PHYSICAL SCIENCE.**

Vol. III.]

MONTREAL, MAY, 1847.

[No. 1.

ART. I.—PROPOSITIONS ON THE “FALLACIES OF PHYSICAL DIAGNOSIS IN DISEASES OF THE CHEST.” By THOMAS ADDISON, M.D.

*Critically Examined by ROBERT L. MACDONNELL, M.D., Lecturer on the Institutes of Medicine, McGill College, Physician to the Montreal General Hospital, Consulting Physician, Montreal Eye Institution.*

In the last number of “Ranking’s Half-Yearly Abstract,” the reader will find a series of propositions from the pen of Dr. Addison, Physician to Guy’s Hospital, London, purporting to point out numerous errors in diagnosis, which those who practise auscultation and percussion are liable to commit, if too exclusive reliance be placed on physical signs.

We do not deny, that the science of auscultation is imperfect, but we do maintain, that without its assistance, we cannot have accuracy in diagnosis, and consequently success in the treatment of thoracic diseases. We have always insisted upon the necessity of comparing the general symptoms, the history of the case, and the mode of succession of the physical phenomena, with the signs actually existing, as indispensable to accuracy of diagnosis, and in this, we have but followed the example of the many distinguished writers who have devoted attention to this subject.

But, in reading the aphorisms of Dr. Addison, one would suppose that auscultators invariably made a diagnosis from physical signs *alone*, and not from a comparison and combination of these signs, with every other particular, capable of elucidating the nature of the malady. Auscultators do not make a diagnosis because they *hear* certain abnormal sounds, but because they *reason* on the physical changes which have produced these sounds. If an observer be perfectly ignorant of the necessity of studying the modifications and combinations of physical signs; the importance of comparing the sounds heard in diseased parts, with those produced in a healthy or less diseased portion of the lung; the value to be attached to a particular sound occurring at a certain stage of the disease; and, above all, if he be as ignorant of pathology as Dr. Addison takes it for granted that auscultators usually are, then, but only then, are the alleged errors he has pointed out likely to be made.

In the observations we are about to offer, we feel it our duty to expose the many fallacies put forward by Dr. A., and in doing so we shall follow him through each proposition, and as briefly as possible, reply to his alleged objections to physical diagnosis. We would, however, observe, *in limine*, that Dr. A. commences with objections to the stethoscope, as if auscultators never employed percussion; and he then objects to percussion, as if those who practise it, never use the stethoscope. His object is but too apparent. He has proposed to himself the task of underrating the stethoscope, and, where the attempt can be made, he neglects not the opportunity—seemingly not aware, that in his efforts, he displays remarkable ignorance of the actual state of our knowledge as regards physical diagnosis. The truth of what we now state, we hope to be able, satisfactorily to prove, and we at once proceed to our task.

A few of the propositions have been so glaringly absurd, that Dr. Ranking has himself pointed out their refutation. We regret he did not criticise more closely the remaining ones; for doubtless, many an inexperienced physician has already been deceived by Dr. A.’s plausible sophistry.

1. It is well known that many persons while under examination entirely fail to perform the respiratory act efficiently, either from nervousness, or from mistaking the manner of accomplishing it. This may lead to an erroneous belief, that the respiratory murmur is deficient, or even absent, while the lungs are perfectly healthy.

This source of fallacy is avoided, says Dr. Ranking, by desiring the patient to cough, and to inspire deeply, so as to cough a second time. This done on both sides of the chest, the actual state of either lung may be ascertained with tolerable precision.

It could hardly have occurred to any writer, except one whose object was to undervalue the stethoscope to urge such an objection. It is, in fact, tantamount to this, that an objection to the use of the stethoscope consists in the *necessity of learning* how to employ it; for if this preliminary step be taken, the above objection falls to the ground.

2. Whatever lessens the freedom, mobility, or elasticity of the ribs, renders the sound on percussion more dull. Hence it is that in rickety persons, where deformity of the chest has taken place subsequent to birth, the signs furnished by percussion are often extremely unsatisfactory; and, indeed, under such circumstances, neither percussion, nor in many instances auscultation, can be much relied upon.

Admitting the truth of this proposition, the rarity of the cases to which it applies, weakens its value; and,

besides, the same alteration of the chest affects not only the physical signs, but also the *general symptoms* of thoracic disease; for in such cases we have difficulty of breathing, cough, palpitations, and congestion of the lips and extremities, owing to the embarrassment to the circulation through the heart and lungs resulting from the malformation.

3. Some persons with actual deformity have naturally such fixedness of the ribs, that they at all-times manifest very imperfect resonance, as well as considerable febleness of the respiratory murmur.

The remarks upon the second proposition apply equally to this one.

4. The rigidity of the cartilages of the ribs in advanced life has a similar effect; and, moreover, often tends to throw obscurity over hypertrophy of the heart, by preventing the usual heaving of the ribs at each systole of the hypertrophied organ.

The answer to this is easily given, by proposing two questions—Is heaving of the ribs truly pathognomonic of hypertrophy of the heart? Have we not more unequivocal signs of this lesion? If so, we can dispense with one of questionable value.

Had Dr. Addison consulted a small work on auscultation, written by one of his colleagues, he would have found the following passage, showing the importance which auscultators attach to mere impulse, as a sign of hypertrophy:

"But does—the student may inquire—a powerful impulse necessarily indicate hypertrophy? No! Is, then, hypertrophy always accompanied with a strong impulse? Assuredly it is not. In explanation of the negative to the former question, it may be stated that an aneurism of the descending, or even of the left side of the ascending aorta, may push the heart aside, and by dilating and producing partial absorption of the parietes, may communicate a powerful heaving impulse to the part in which the natural impulse is felt.

"In explanation of the negative to the second question, it may be observed, that though the heart be powerful, and the parietes of the ventricles very thick and strong indeed, the action of the heart may be so hampered by the consequence of obstruction in the valves, and its contractions may be so impeded, and so overpowered by fluid accumulated either within or without its cavities, as to be almost entirely mastered, and to be only just enabled to flutter a little, and thus to rid itself of its load. Such, indeed, is very commonly the condition of the organ in long standing diseases, of the valves, and particularly in that of the mitral valve, when the nervous energy of the individual is considerably reduced; even though the muscular power of the heart, abstractedly considered, be much greater than in health.

"Such is commonly the condition of the impulse in the last days of the life of persons affected with disease of the valves, accompanied with hypertrophy. If they do not die suddenly, as persons so affected frequently do, it may indeed be considered to be the natural termination of such cases.

"A very large and powerful heart, therefore, may be accompanied by a very feeble, irregular, and fluttering impulse; and a very small and feeble heart may be, and usually is, accompanied with a very smart and 'snacking' one."—*A Clinical Introduction to the Practice of Auscultation*; by H. M. HUGHES, M. D., p. 198.

5. When exploring the chest in a case of recent disease, we may be misled by the permanent effect of an ancient pleurisy.

No one in the habit of making examinations of the chest, and familiar with the pathological changes result-

ing from ancient pleurisy, can be easily deceived by the phenomena alluded to.

6. When, as usually happens, rickety deformity of the chest consists in lateral flattening of the ribs, with projection of the sternum, the action of the heart is liable to beat with such violence, and over so diffused a space, as to lead to the unfounded apprehension of organic disease of the organ:

True; but in such cases the diagnosis can generally be made from the presence or absence of more unequivocal signs of organic disease. Dr. A. seems to attach by far too much importance to increased action, as pathognomonic of organic cardiac disease—a mistake exceedingly general.

7. The dullness on percussion, caused by pushing up of the diaphragm by an enlarged liver, or fluid in the peritoneum, is liable to be mistaken for dullness caused by fluid in the pleura.

Such a mistake can only be made by an examiner who has paid but little attention to the progress of our knowledge on this subject. The diagnosis between these affections was clearly pointed out by Dr. Stokes many years ago, and has been so lucidly explained by subsequent writers, that it is very unlikely that, with ordinary care, such a mistake can be made. In any case, the objection only applies to the right side of the chest.

8. Bronchitis is a frequent source of fallacy, it may greatly obscure pneumonia, phthisis, and pleurisy, as well as other chronic diseases of the organs.

Every pathologist is aware of this; yet we do not recollect having, in our (by no means limited) experience, found any difficulty in diagnosis from this cause. On the contrary, the existence of bronchitic râles have, in more instances than one, greatly assisted in establishing the diagnosis, as, for instance, in cancer of the lung, emphysema, &c. We have no hesitation in stating that an experienced auscultator will suffer but little annoyance from this alleged source of fallacy.

9. When the bronchitic complication of phthisis is considerable, we often fail to detect some or all of the physical signs of the latter, such as dullness on percussion, tubular respiration, and even bronchophony and pectoriloquy. This is more especially the case in the earlier stages.

Dr. Addison seems to forget that a bronchitic râle, accompanied by dullness, is one of the most valuable signs of the first stage of pulmonary consumption. This was shown many years ago by Dr. Stokes, and it has more recently been insisted upon by Mons. Louis. In another place we have stated that "A bronchitic râle, confined to the upper lobe of one or both lungs, resisting treatment, and accompanied or followed by dullness, at first slight, but gradually increasing, is as valuable a physical sign of phthisis as any we possess." So far, then, from bronchitic râle obscuring the signs of phthisis, it is, on the contrary, one of the most valuable indications of the early stages of that disease, particularly if occurring at the apices of the lungs. Moreover, it is not easy to

understand how a bronchitic r le, produced by the passage of air through diseased bronchial tubes, can mask dullness on percussion. But even supposing it capable of doing so, could not percussion be practised whilst the patient is made to keep in his breath. Dr. Addison states that his proposition applies particularly to the earlier stages of phthisis; but surely he does not consider tubular respiration, bronchophony, and pectoriloquy, characteristic signs of the earlier stages. The first two are never heard in the earliest stage, and the latter is never heard but in the last stage; consequently a bronchitic r le cannot be considered as a fallacious sign, but one of extreme value in the earliest stage of phthisis—the only period in which our efforts to arrest the disease, are likely to be attended with success.

10. Dullness of sound on percussion, tubular respiration, bronchophony, pectoriloquy, and gurgling, are not necessarily conclusive of phthisis. All these signs may result from changes induced by a former pleurisy, from pleuro-pneumonia, or whooping cough, or even from recent pneumonia or pleurisy associated with considerable bronchitis.

The diagnosis is not so difficult as seems implied in the above statement. When pectoriloquy and gurgling are heard in pleuritic effusions, they are accompanied with other unequivocal signs of that affection, as for instance, dilatation of the side, protrusion of intercostal spaces, displacement of the heart or liver, loss of vocal vibration, &c., and they indicate the removal of the fluid of an empyema, through a perforation in the lungs, and consequently are attended by signs of this communication, such as “splashing,” metallic tinkling, amphoric cough, and pneumo-thorax. The possibility of confounding cases of phthisis, in which the physical signs alluded to, are presented, with pleuro-pneumonia, we do not deny, if no attention be paid to the history of the case, the rapidity of progress, and the presence or absence of other signs of phthisis.

We have never heard dullness of sound on percussion, tubular respiration, bronchophony, pectoriloquy, and gurgling in whooping cough; and, until we saw the present proposition, were always under the impression that the last mentioned disease was remarkable for the absence of physical signs. A view in which, we believe, almost all observers coincide.

11. When, in phthisis, the larynx is so involved as to impede the entrance of air, and give rise to permanent sonorous r le in the tube, the reverberation of this r le through the entire chest is apt to lead to the erroneous suspicion of disease in the lungs.

This statement is open to two objections. In the first place, if the disease in the larynx have advanced so far as to impede the entrance of air, the quantity of air thus admitted is not likely to cause much reverberation. Secondly, Dr. Addison must be aware that in phthisis the laryngeal symptoms rarely set in till towards the close of

the disease, when, of course, no embarrassment in diagnosis can arise from the occurrence of r le. He must also be aware, that even supposing his statements true, (which we are far from allowing), the occurrence of such a complication must be so rare, that none but a prejudiced writer would urge such an objection to physical diagnosis.

12. Complete loss of voice from disease of the larynx almost completely nullifies the results of auscultation.

When loss of voice occurs as a complication of thoracic disease, the latter has always preceded the former. We know of no case where aphonia has obscured the diagnosis, but we know of many where it is of the greatest service in assisting us to form a correct one.

In any case, loss of voice can only affect the *vocal* auscultatory signs, and these are so perfectly valueless, *taken by themselves*, that the auscultator can, without disadvantage, dispense with them. We know of no signs which so constantly mislead the inexperienced, as the different modifications of the voice. A slight resonance, so common in women and young men, with shrill voices, is constantly put down as pectoriloquy and bronchophony. It is now generally admitted that Laennec attached too much importance to these signs. We wonder that Dr. A. did not at once discard them as useless complications.

13. The existence of a cavity may be overlooked if the bronchial tubes leading into it are plugged with mucus.

In every case of suspected phthisis, says Dr. Ranking, the patient should be made to breathe and cough with violence; this will dislodge mucosities and render the existence of a cavity perceptible.

Has Dr. A. ever known a mistake made under the above circumstances? It seems to us that the merest tyro would have desired his patient to cough—a simple and ready method of dislodging the plug of mucus, the cause of so much confusion.

This fallacy seems to have suggested itself to the Doctor at his desk, and not at the bedside.

14. A patient may have all the rational signs of incipient phthisis, while auscultation does not reveal any change in the lungs.

Similar symptoms may arise from relaxed uvula, and in hysteria.

In a note appended to an essay on empyema, published four years ago, we stated—

“I am not ignorant of the fact, that in some rare cases of phthisis, the constitutional symptoms may continue for a long time before the slightest traces of the physical phenomena of the disease become manifest, owing, most probably, to the morbid processes being confined to the central parts of the lung. I have now seen many such cases, and have observed in some of them a peculiarly fetid odour from the breath, after coughing, and from the expectoration. The diagnosis, in these obscure cases, rests upon the want of correspondence between the presence of all the symptoms of phthisis and the total absence of the physical phenomena. We are not, however, in such cases, left long in doubt, for very soon the lesion becomes discoverable by auscultation and percussion.”—*Dublin Med. Jour.*

We do not contend with Dr. A. for the originality of the proposition; but though we have, even more fully than he, stated our experience of these cases, we never fancied that because they form a *rare exception*, he or any one else should have considered them as invalidating the *rule*.

15. Dilated bronchial tubes surrounded by indurated pulmonary tissue, cannot be distinguished from phthisical lesion by auscultation alone, especially if situated in the apices of the lungs. In such cases the diagnosis is chiefly formed by the history of the case.

It is true, that a careless or inexperienced auscultator may not be able to make the diagnosis, for there are many signs common to the two affections. In both, we may have dullness on percussion, pectoriloquy, gurgling, and absence of respiratory murmur; but in dilated bronchial tubes, *there is a want of accordance between those signs and the general symptoms*. There is no hectic emaciation, or rapid sinking, as in phthisis. Moreover, the history of the case shows us clearly that its slow progress is not reconcilable with our notions of the course of phthisis; and this view is still further strengthened by the fact, that in dilated tubes the *physical phenomena remain unchanged for years, whereas, in phthisis, their mutations are rapid and progressive*. There are other points of dissimilarity, but enough has been stated to prove that, with ordinary care, the "fallacy" alluded to, may be avoided.

16. Malignant disease of the lungs cannot be distinguished from other lesions by auscultation alone.

True; but does Dr. A. pretend that the diagnosis can be made *without* auscultation? If so, he is highly culpable for keeping his professional brethren ignorant of this important fact. It is certainly quite true, that we cannot rely solely upon physical signs for a diagnosis of malignant disease of the thoracic viscera. But has any writer on the subject maintained that *they alone* are sufficient for that purpose? Do the observations of Stokes, Walshe, Taylor, or Graves, assert the affirmative of the proposition? Certainly not; they, and all subsequent observers, have insisted on the necessity of connecting the general symptoms with the physical phenomena, and thence deducing a diagnosis. It had been more wise had Dr. A. waited until auscultators *had asserted* what he has undertaken to *disprove*.

17. If acute pneumonia have proceeded to complete hepatization when we first examine the patient, the physical signs are frequently insufficient to distinguish it from tubercular consolidation or ancient pulmonic induration. This is especially the case if the apex of the lung be the seat of the induration.

That it is extremely difficult, indeed we may say impossible, to distinguish complete hepatization when we *first* examine the patient, from tubercular consolidation

or ancient pulmonic induration, we freely admit. But we would ask, does Dr. A., or any other physician of equal experience, always make a diagnosis on the first examination of his patient, without inquiring into the origin, mode of succession, and modification of the symptoms and physical signs.

If we neglect to inquire into these particulars, and rely on physical signs alone, we shall not be able, from their assistance, to make an accurate diagnosis; but if we recollect that pure pneumonic consolidation is by no means frequent at the *apex of the lung*, and that "ancient pulmonic induration," or, in other words, "*carcinification*" of the lung is a condition of the organ very rarely observed, and *par consequens*, still more rarely met with at the apex of the lung, it must be admitted that the value of proposition, No. 18, is very questionable.

18. Pneumonia may occur without cough, and so closely resemble simple continued fever that both the stethoscopist and the non-stethoscopist are apt to be deceived.

Of all cases in which the stethoscope has been employed, its vast utility has not been more indisputably proved, than in the very instances alluded to by Dr. A., namely, *latent pneumonia*; in such a case we unhesitatingly assert that *without the stethoscope* a correct diagnosis *cannot be made*; and if the disease has been overlooked by Dr. A., or any physician, it is because auscultation has not been employed. The reader will observe, that Dr. A. started with showing how the use of physical signs may mislead the practitioner; in the above proposition, he inadvertently points out how we may be deceived by not employing them. He has, unintentionally, proved a little too much.

19. When the anterior and inferior portion of the left lung is consolidated by pneumonia, it may not be detected by percussion on account of the proximity of a flatulent stomach. Under similar circumstances a marked amphoric respiration is produced, with metallic tinkling, leading to the erroneous conclusion, that pneumo-thorax is present.

We may here observe that solidification of the anterior and inferior portions of the lung is not very common; indeed, we doubt much if Dr. A. has met with many examples of it.

Be that as it may, we would ask Dr. A. has he known the mistake alluded to, to have been frequently made?—or has he not more generally remarked, that even inexperienced auscultators recognize the source whence the amphoric sound proceeds. Under such circumstances we have frequently made the patient swallow a few drops of water, in order to instruct a class in the detection of these sources of fallacy; the water dropping into the flatulent stomach produces a sound closely resembling metallic tinkling; but we do not recollect to

have heard metallic tinkling spontaneously produced in these cases, as seems implied in Dr. A.'s remarks.

In any case, a few drachms of any carminative mixture, by expelling, or displacing the flatus, will remove the source of error.

20. It cannot be determined by physical examination whether pneumonia have or have not supervened upon tubercles, although the prognosis in the two cases would be very different.

With due deference, we do not see how the prognosis can be much affected in the manner alluded to by Dr. A. For if we are satisfied that phthisis is unquestionably established, the supervention of pneumonia can only affect the prognosis, as far as the probable duration of life is concerned, it does not render the disease more fatal, though it no doubt shortens the duration of the sufferer's existence.

21. I doubt whether physical examination can in any instance determine with certainty, the existence of simple tubercles in the lungs.

We do not purpose trying to convince Dr. A.; we would, however, advise him to apply himself to this branch of auscultation; and as he appears to be far behind the age, we recommend to him the manual published by Dr. Hughes, his colleague.

22. When serous effusion is very considerable, giving rise to unequivocal bronchophony, tubular respiration, and want of resonance and vocal vibration, physical examination has repeatedly led to a mistaken belief that these signs resulted from pneumatic or other consolidation of the lung.

Setting aside altogether the fact that when serous effusion advances to the extent alluded to by Dr. A., it generally produces a displacement of the heart, to the right side, when the effusion is situated on the left side of the chest, and a displacement downwards of the liver, when the effusion occurs on the right side: it must not be forgotten, that enlargement of the side and bulging out of the intercostal spaces, signs so characteristic of extensive serous effusion, are never noticed in pneumonia; but as they occur in some cases of cancer of the lung, the diagnosis might be rendered obscure. As I have already drawn the attention of the profession to the points which will enable them to form a correct diagnosis in such cases, I shall content myself by merely alluding to those observations. A consolidation of the lung from pneumonia never yields absolute dullness on percussion, and as this sign attends all cases of pleuritic effusion of any extent, it alone would serve to distinguish the one from the other.

Besides, in extensive pleuritic effusion, there is complete absence of tussive and vocal vibration over the affected part, signs which are never absent in pneumonia.

23. When a patient presents himself with febrile affection of any kind, we may, on examination, detect dullness or percussion, tubular respiration, bronchophony, and a rale not distinguishable

from the submucous crepitation commonly observed in pneumonic hepatization; and yet physical examination should not enable us to determine whether the chest affection be recent or of ancient date. When a portion of lung has been compressed by pleuritic effusion, and has been prevented from expanding again by adhesions, the physical signs may remain permanently, and be found to resemble precisely those which result from recent pleuropneumonia.

We are sorry to appear so captious, but we really cannot allow another gross blunder to pass unnoticed. Firstly, we maintain that submucous crepitation is *not* heard in hepatization of the lung, but *after* the hepatization has commenced to pass into the stage of resolution. Secondly, "when a portion of the lung has been compressed by pleuritic effusion, and *has been prevented from expanding again by adhesions*—a process of contraction commences in the parietes of the affected side, which quickly and very perceptibly produces a flattening of the chest, corresponding to the seat of the disease, accompanied, moreover, by depression of the shoulders, and tilting outwards of the angle of the scapula.—Has Dr. A. ever seen such consequences follow recent pneumonia? or is *he* in the habit of making a diagnosis without inquiring into the history of the case? if so, we wonder not at his alluding to sources of fallacy, which we venture to say, no auscultator but himself ever encountered. We are not surprised, that if Dr. A. attach but as little importance to the pathology of thoracic disease, and to the order of succession, combinations and modifications of physical science as it appears he does, that he should have derived but little assistance from the stethoscope.

24. Experience leads me to the conclusion, that pleuritic friction-sound cannot in all cases be distinguished from the rubbing produced between the inflamed peritoneal surfaces of the liver and diaphragm; neither can the croaking sounds produced in the bronchi be always distinguished from the pleuritic rub.

Admitting the probability of the error alluded to, in the first part of the above sentence, (although it has never occurred to us to meet with friction sound, arising from the rubbing of the inflamed peritoneal surfaces of the liver on the diaphragm, except when the liver presented tumours on its surface), yet it can only occur on the right side. Sometimes there is, no doubt, difficulty in discriminating between the rubbing sounds and those generated in the bronchial tubes, yet the accompanying symptoms and the history of the case will always enable us to distinguish the one from the other.

25. A simple pericarditis is rarely attended with pain, and as the other symptoms of that disease are equivocal, the physical signs are chiefly to be relied upon in forming a diagnosis. Nevertheless, when effusion has taken place to a certain amount, the friction-sound commonly disappears, and auscultation fails to recognise the disease.

If friction sound has been heard in a case of pericarditis, and has suddenly disappeared, the change indicates, either a return to a perfectly healthy condition, adhesion

of the opposed surfaces of the membrane, or effusion of serum into its cavity. If the first or second of these changes has taken place, the sound on percussion over the heart will present its usual character; but in the latter case, the loss of friction sound is followed by *extensive, complete, and absolute* dulness all over the cardiac region, and even beyond it. Being aware of these facts, we are not likely to be misled by the disappearance of the rubbing sounds.

26. Enormous accumulations of fluid in the pericardium, cannot always be distinguished from effusion into the cavity of the pleura.

Here is another great error.

Has Dr. Addison ever tried the diagnostic test of Laennec for effusions into the pleura, viz: the changing of the position of the patient? if so, how can he suppose that we are to accede to the foregoing proposition. Effusion into the pericardium *cannot be displaced* by the position of the patient, but that which is produced by the pleuritic inflammation readily changes its position, if that of the patient be changed. Moreover, as before stated, extensive effusion into the left pleura commonly produces dislocation of the heart to the right side. Has Dr. A. ever known this phenomenon produced by effusion into the pericardium? we answer no! Again, effusions of great extent into either pleura are usually attended with dilatation of the *side*—protrusion of the intercostal spaces, œdema of the integuments (frequently) and loss of vocal and tussive vibration—signs which are not noticed in pericarditis with effusion.

27. When the pericardial friction-sound is single, auscultation may fail to distinguish it from a valvular murmur, especially if it be situated over the region of the valves.

To the young stethoscopist this difficulty might present itself, but the history of the case, the long existence of cardiac disease, the absence of febrile symptoms and of pain over the cardiac region, &c., would point out the difference between an old and recent attack of the heart. Surely Dr. A. does not mean to assert that it is common to find valvular murmur as rough as friction sound, and still remain single! But even so, there are many distinguishing marks between them. Valvular murmur, when single, is always situated over one or other set of valves, strictly accompanies the first sound, is not influenced by position or by pressure of the thoracic walls against the heart, conveys to the ear the idea of being generated deep in the substance of the heart, and is not removed or modified quickly by treatment—in all these particulars a strong contrast exists between the two sets of sounds; moreover, friction sound is superficial, and is increased by making the patient lean forward, (*i. e.*, if no effusion have already taken place into the sac) so as to bring the opposed surfaces of serous

membrane into close contact, whereas valvular murmur remains unaffected by these manœuvres.

28. The double pericardial friction sound may be confounded with the see-saw murmur of imperfect aortic valves, and vice versa.

The diagnosis of disease of the aortic valves does not rest solely on the see-saw sound—nor has any auscultator asserted that it did. Besides that sound, we look for the bruit de soufflet in all the arteries of the upper extremities, that sound proceeding from the aorta to the smaller branches: the visible pulsation of the vessels of the neck and arm, and the jerking pulse at the wrist, or as Dr. Hope termed it, the “pulse of unfilled arteries.” We have also combined with these, evidence of hypertrophy of the heart in many cases. Moreover, the see-saw sounds are most intense over the aortic valves, whilst those from pericarditis are most intense corresponding to the junction of the auricles with the ventricles. It will be noticed, that Dr. A. again assumes what we have so frequently contended against, that auscultators rely solely on physical signs, for he must be aware, that the history of the case, will, in many instances, afford assistance in our diagnosis.

29. A sound closely resembling a murmur appears sometimes to be produced by the stroke of the heart against a portion of lung interposed between it and the parietes of the chest. Under such circumstances, auscultation may lead to the erroneous conclusion, that the heart is diseased.

As we have never heard this sound, we make no remarks upon the above proposition.

30. Auscultation fails to distinguish an aortic murmur depending on organic change from one which results from other causes; neither can it decide whether what has been called a mitral murmur is organic or functional.

We admit that, *occasionally*, difficulty does exist in making a differential diagnosis in the cases alluded to, but auscultators have always (except, perhaps, Dr. Hope), admitted this difficulty; yet here also, attention to the accompanying symptoms, the origin of the disease, and above all, the changes produced by exercise, and medical treatment, will enable the practitioner to arrive at a correct conclusion.

31. In certain diseases of the heart it is difficult or impossible to localise the murmurs with accuracy, however pronounced they may be.

We also admit the difficulty, but are the prognosis or treatment affected by it?—provided we are enabled to distinguish between organic and inorganic diseases of the heart, we hold, that the refined diagnosis between an affection of this or that valve, however useful in establishing accuracy of observation, is but of little importance in practice.

32. Auscultation cannot distinguish the murmur of an aneurismal artery from the murmur produced by external pressure upon the vessel.

This has also long since been admitted by stethoscopists; but is the diagnosis imperfect?—by no means.

33. Physical examination does not enable us to distinguish congenital malformation from disease of the heart or large vessels.

Physical examination fails here, and so do general symptoms: congenital malformation may be guessed at, but cannot, with accuracy, be detected.

We have now performed the task we proposed to ourselves, and we dare say, our readers will agree with us, that a more agreeable one, might have been selected; but we felt it our duty to expose the shallowness of argument, the very evident special pleading, and above all, the assumption on the part of Dr. A., that all his readers are as little acquainted with the present state of auscultation, as we trust we have shown him to be. The *ignorantia elenchi*, is the species of reasoning, upon which Dr. A., evidently relies in his argument throughout. That Dr. A. has rendered great service to the cause of science, we gratefully acknowledge; that he may be an excellent practical physician, we readily admit—but that he is a sound or skillful stethoscopist, we very much doubt. The stethoscope has had many enemies to encounter; but we know of none, pretending to familiarity with its employment, who has made such a determined effort to throw discredit upon it, and to discourage others from learning its application.

Montreal, April 15, 1847.

#### ART. II.—CONTRIBUTIONS TO CLINICAL MEDICINE,

By J. CRAWFORD, M.D.,

Lecturer on Clinical Medicine and Surgery, McGill College.

Case—Perforation of the Appendix Vermiformis—Peritonitis—Death.

Bridget Keer, a single woman, aged 25, came under my care in November 1846. The brief history of her case then given was, that she had been an inmate of the Montreal General Hospital, (with the exception of a short interval), ever since January of that year, for diarrhoea; and that she had on many occasions passed blood per anum, her dejections being also frequently mixed with puriform or muco-purulent matters, but that in general they were thin and watery. She had laboured under her present complaint for four months previous to admission. She was much reduced in flesh and strength, and at several periods she had suffered from hectic fever and profuse perspirations. Her abdomen was generally more or less painful, and she suffered occasionally from colic, or cramp in the right iliac region. During the whole period of her illness, she had not menstruated. She had been treated by a variety of ways,—astringents, iron, sulphate of copper, opium, &c. &c., were tried with occasional temporary advantage.

In the beginning of November, her complaints were very troublesome, her diarrhoea frequent, the evacuations being attended by griping, and the abdomen generally tender on pressure.

Her weak state confined her to bed, and she was subject to nervous palpitation in the epigastrium and course of the aorta. Her pulse 104, small; tongue clean and moist. She had pain of the spine, in the region of the 4th dorsal vertebra, in the loins, and also under the left mamma. She was ordered a blister to the abdomen, and an astringent mixture. This afforded her temporary relief from the abdominal pain and diarrhoea. She was then ordered decoction of cinchona, and also Griffith's mixture, in succession. Under this treatment she appeared to improve, but occasionally had severe returns of her complaints. By degrees, however, she recovered strength, and generally, throughout the month of December, could sit up a little daily. During January, although she was still subject to severe attacks of diarrhoea, (for a day or two at a time), she continued to improve, and became fat; and during the principal part of February, she seemed nearly free from disease. On the 2d March she was reported to have had several dejections, mixed with blood, on the preceding night, which were also attended by severe colic pains. A blister was ordered to the abdomen, and sulphate of copper with opium, (of each gr.  $\frac{1}{2}$ .) every four hours. Next day it was stated that she had been vomiting for most part of the night, and that her bowels had been much disturbed, accompanied by severe abdominal pain; she was also very weak and low. At the visit, she appeared to be suffering much from abdominal pain, and could not bear any pressure on the part. There was great anxiety of countenance; pulse 130, small and hard; skin cold; face pale, with cold perspiration over the forehead.

The symptoms indicated that there was peritonitis, probably arising from perforation of the intestine. Opium, in grain doses every two hours, was ordered, and the cupping glasses to the abdomen. The vomiting continued, and did not appear to be influenced by the remedies; she progressively and rapidly became worse, and died at 3 A.M. on the 4th, about 24 hours from the commencement of the vomiting and indication of peritonitis.

*Sectio cadaveris.*—The body *en bon point*. The abdomen distended with gas. The parietes of the abdomen appeared much loaded with fat, which was upwards of an inch in depth; in like manner the omentum was fringed round its loose edge by large masses of fat. The intestines and omentum, were in a high state of inflammation, being generally of a bright rose colour, and in some parts of a lurid red, as if scalded, particularly in the vicinity of the cæcum. Over several parts there



was an effusion of plastic lymph, which agglutinated the contiguous portions of intestine together, wherever they came in contact. The peritoneal lining of the abdomen, (particularly in the vicinity of the right ilium), was very red, like the intestines. About half a pint of puriform looking yellowish fluid was found effused in this region. The cæcum had become extremely attenuated, and in most parts the peculiar structure of the mucous tissue had disappeared, the serous coat alone seemingly remaining. The bowel appeared as thin and diaphanous as silk paper; there was no marks of ulceration or erosion, but rather a general atrophy. On removing this bowel, it was found to be adhering to the iliac muscle, and a large oval opening was torn in it, although care was taken in removing it. The appendix vermiformis was about an inch and a half long, and about three quarters of an inch broad; its coats were likewise thinned: a small ulcer was found towards its upper part that would admit the passage of a goose quill. The opening appeared filled with a pulpy matter, and the edges were even. The uterus and ovaria partook of the general inflammation, but were in other respects normal.

The cæcum and appendix vermiformis contained a gruel-like fluid, resembling, in some degree, that effused into the peritoneal cavity, but not of the same yellow colour. There were not any ulcerations observed in any part of the intestinal canal, although there were in some parts discolorations, which might represent the situations where ulcerations might have formerly existed. The coats of the bowels were generally thin, but not so much attenuated as the cæcum.

*Remarks.*—The great accumulation of fat in the abdominal parietes, and on the omentum, was remarkable and unexpected in this case, where a tedious and wasting disease had existed so long, and where such atrophy of the intestinal mucous membrane appeared. Similar accumulations of fat have, however, been observed by Dr. Budd and others, where great emaciation prevailed, in cases of prolonged dysentery, in stricture of the pylorus, and in phthisis pulmonalis. No very satisfactory explanation has yet been offered of this pathological condition. It is probable that the posterior portion of the cæcum would have ruptured, if it had not formed adhesions with the subjacent parts.

#### ART. III.—CASE OF HYDROPS OVARII.

By WILLIAM MARSDEN, M.D., Nicolet.

The following case of hydrops ovarii, of which I was requested by the friends to make a *post mortem* examination, assisted by Drs. Joseph O. Rousseau, and P. Brassard, is sufficiently interesting, both pathologically

and physiologically, to ask a place for it in the columns of your very valuable and useful journal.

Josephite Bonneville, widow of Joseph Lemire, of St. Esprit, died on the 24th of February, 1847, æt. 54. On the 25th, 24 hours after death, made the *post mortem* examination. The external dimensions of the body were as follows, notwithstanding, there was great emaciation, and a considerable discharge of fluid had taken place, completely saturating the bedding and floor:—circumference of the body, over the umbilicus, four feet ten inches;—the circumference of the tumour, horizontally round the scrobiculus cordis, sides of the abdomen and pubis, five feet one inch. Perpendicularly, from the sternum to the pubis, two feet nine and a half inches.

On opening the cavity of the abdomen, it was found to contain an enlarged ovarium, completely displacing all the other viscera. The diseased ovarium was of the left side, and was adherent to the peritoneum throughout the greater part of its extent; and to the diaphragm, transverse colon and bladder. The ensiform cartilage, stood nearly at right angles with the sternum; and when the tumour was removed from the body, the inferior margin of the ribs gaped like the mouth of a funnel. A line drawn from the anterior inferior angle of the ribs, measured eleven and a half inches. On removing the tumour from the body, it was found to weigh 71 lbs. It was of the encysted variety, and divided into about eleven principal compartments; which were again subdivided into many smaller ones, and contained fluids of different kinds. One large one contained upwards of two pounds of serofulous looking matter; others contained a dark yellowish brown serum, mixed with slime; and others again, a ropy kind of jelly, or a brownish looking kind of pus. The tumour or carneous mass, when emptied as far as practicable, by puncturing the cavities, weighed 45 lbs. The abdominal cavity contained a large number of hydatids, varying in size, from a few drops, to two ounces, which were attached by long pendulous necks to such portions of the peritoneum as were not involved in the tumour. In shape, they resembled a Florence flask, or rather a Riga balsam bottle, somewhat flattened on one side, and broader at the base than the shoulders, but much longer in the neck, some of the smaller ones being five or six times as long as the body. The hydropic tendency was general, as that portion of the cavity of the abdomen, which was unoccupied by the tumour, contained five and a half gallons of high coloured serum. The cavity of the thorax also contained six pints of bright yellowish green looking serum. The bladder was empty, and its walls extremely thin, and flattened out to a diameter of about eleven inches. The great omentum was so completely



involved in the diseased ovarium & scarcely to be distinguished, except as forming a bond of union with the transverse colon. The uterus was elongated and fusiform, the point being toward the fundus uteri, and about five inches long; and the broadest part, about an inch in diameter, near the cervix uteri. Could distinguish remains of the right ovarium; but no traces of the fallopian tube of that side. The left fallopian tube was considerably elongated, widened, and ramified over the side of the tumor. The concave surface of the stomach, and the duodenum and jejunum bore inflammatory traces, and especially the latter. The right kidney was little more than half its natural size; but the left one was of the usual size, and healthy. The spleen was enlarged and unusually light coloured—being nearly the colour of the liver. The mesenteric glands were enlarged and schirous. All the intestinal canal, excepting the portion above described, healthy and empty; excepting the rectum, which contained a quantity of greyish looking, indurated feces; the liver remarkably healthy looking, and the gall bladder empty. The lungs healthy looking—but the left one had several adhesions to the diaphragm.

The subject of this disease was about 5 feet 7 inches high; of bilious nervous temperament; of excellent habit of body; and had not been subject to disease of any kind, until after the cessation of the catamenia. She is the mother of one child—a son, now 34 years of age. Her own history of her case is as follows:—She married young, menstruated regularly and early, although sometimes copiously, until she was 45 years of age. Had only one child and never miscarried! Her general health continued good for some months after the catamenia had ceased, when she was attacked with dysuria, with pains in the back and loins, as she used to be when the menses were going to flow; and at the end of two years, imagining that such was about to be the case, and that if so, she should enjoy better health, she placed herself in the hands of an empiric, who guaranteed that he would restore the secretion; and under his treatment had one change, which lasted several days. After this, she began gradually to increase in size, and her husband being then alive, she imagined, until upwards of nine months after, that she was pregnant. It was only after this, that she stated her case to Dr. Rousseau, who was to have been her accoucheur, and who at once suspected the nature of it.

Her general health, notwithstanding the very great size she had attained, continued almost unimpaired, until last autumn, when her appetite began to fail, and she visibly lost strength and flesh; but the animal spirits were, until within a few weeks of her decease, uniformly

buoyant or cheerful. At that time (last autumn) she began to find her size inconvenient, though not painful; previous to that she had continued to walk out alone. Subsequently, however, she became subject to constipation, irritable bladder, &c. Until four weeks before her decease, she walked about without help; and on the Sunday previous to her decease, she walked about her room with a person to assist her.

This case affords several points for physiological reflection, when coupled with the singular fact, that *she leaves a sister, two years older than herself, who is similarly afflicted; and a maternal aunt who has been labouring under an enormous abdominal enlargement for 15 or 16 years past.* The medical attendant of the latter, Dr. Alexander, jr., of La Baie du Trebone, whom I addressed on this subject recently, states her case to be uterine.

The above case, in some respects, is analogous to one of Sir L. M'Lean's, reported by Good, in Vol. v. page 422, of a lady who died "from the bursting of the abscess into the peritoneal sac." On examining the body, he says, two pints of a "thick brown, well digested pus were found to have escaped into the cavity of the abdomen; and three pints more in the ovarian sac. The opening was large enough to admit of three fingers; and the external surface of both the large and small intestines was found inflamed, and verging, in some places, to gangrene." "This," says Good, "my learned friend ascribes to the influence of the pus that had escaped, and was in contact with them;" but as the fluid is said to have been well digested pus, the inflammation is, I think, more probably to be attributed to sympathy with the lacerated ovarium, in its actual state of irritation from so large a rent, and so much larger an inflamed surface in its interior.

Now, it will be remarked, that the external surface of the stomach and small intestines was found inflamed in Bonneville's case; but the cause, I should rather attribute to the intimate connexion of the peritoneum, which is reflected over, and in proximity with, a morbid organ, in which a high degree of inflammatory action exists; and as directly exciting sympathetic inflammatory action as either lesion or effusion of pus.

On a review of the case, among the questions that present themselves may be asked: What is the remote cause of this disease? and wherefore the apparent hereditary tendency? Can the small size of the kidneys have occasioned or aggravated the hydropic tendency? Was the uterine discharge, (two years after the catamenia had ceased,) not rather hæmaturia? Was the primary disease not a nephritic one? and was it not induced by catamenial metastasis or revulsion? Could

any curative means have been adopted to relieve or arrest the disease?

A remarkable feature in this case, was the very slight constitutional derangement, notwithstanding the long existence of so enormous a morbid formation. The whole weight of the superabundant accumulation in the abdominal and thoracic cavities, could not have been less than 130 lbs. For instance,—

The ovarian tumour, . . . . .	71 lbs.
5½ gallons of serum in the abdominal cavity, . . . . .	44 “
6 pints of serum in the thoracic cavity, . . . . .	6 “
say 10 pints by escape after death, . . . . .	10 “

Making in all, . . . . . 131 lbs.

The escape from the body, after death, could not have been less than from one gallon to one and a half gallons, as the abdomen during life, and immediately after death, was hard and tense; and when examined the day after, it had become comparatively soft and flaccid, so that we could readily distinguish the character of the tumour, by the irregular feeling of the soft and hard parts.

It is worthy of record, and due to the memory of the deceased to state, that it was by her own expressed desire, frequently repeated to the writer, that a *post mortem* examination of her body took place; and it is still more creditable to her feelings, when it is considered that the greatest antipathy and prejudice against *post mortem* examinations exists among the peasantry; and that the majority of her friends and relations were so much opposed to it, that nothing but the fear of disinterment of her body, induced them to accede to her request.

Nicolet, March 15, 1847.

ART. IV.—CASE OF AMPUTATION OF LEG—THE PATIENT UNDER THE INFLUENCE OF SULPHURIC ETHER VAPOUR.

By E. D. WORTHINGTON, M.D., Sherbrooke.

As experiments are being daily made to test the efficacy of the inhalation of the vapour of ether, as a means of preventing pain in surgical operations, will you allow me to contribute one to the list of successful cases.

On Thursday, the 11th, I performed amputation below the knee, assisted by Drs. Andrews and Rogers, of Eaton. The case was one of extensive disease of the ankle joint, involving the bones of the foot, and lower end of the shaft of the tibia and fibula, the result of an accident received seven or eight years ago. The patient, a man aged 30, was quite willing, indeed anxious, upon a fair representation of facts, to try any means that promised to lessen the dreadful pain of an operation. A large ox-bladder, with a stop-cock attached, a mouth-piece, made of thick leather, covered with black silk and well padded round the edges, with a connecting

long brass tube that had done service as an umbrella handle in many a shower, formed an apparatus that, though rude looking, and bearing marks of having been got up in haste, presented withal a very business-like, and, for the country, tolerably professional appearance. A couple of ounces of ether were poured into the bladder, which was then filled with air from a bellows. Not having time or ingenuity sufficient to construct a double valve, the objection to inhaling carbonic acid gas again into the lungs was done away with, by simply allowing the patient, after a full inspiration from the bag, to expire through the nose, for three or four times, when the nostrils were kept closed, and the breathing confined to the bladder. From this time about six full inspirations sufficed to produce a complete effect; the eyes turned up under the upper lid and became fixed; his wrist was pinched, and he was asked if he felt pain; he laughed, and said, "Oh no, I just feel—no pain at all!" The operation was then commenced, and terminated without his evincing, in any way, that he was at all conscious of the least feeling of pain. He retained his consciousness, talked rationally, and made some very witty remarks in answer to questions put to him, converting the scene from one of a most painful to one of an excessively ludicrous character. Both during the operation, and afterwards, he expressed himself as knowing perfectly well what was doing, and the different stages of the proceeding, but at no time did he feel pain. Indeed, after it was over, he kindly volunteered to have half a dozen legs taken off, always provided he was plentifully supplied with the gas. Alternate inhalations of gas with atmospheric air sufficed to keep up the effect, except at one time when he had a "presentiment of pain," and gave the word to "pass the bottle," which he afterwards seemed to cherish as a bosom friend. The effect terminated as the dressings were completed, leaving no visible arguments against the use of ether, and many quieting ones of its advantage. As far as this case goes, it is most convincing, and I offer it as one tending to show that, notwithstanding particular idiosyncracies, in its general application the discovery is one that cannot fail to be of infinite advantage in the hands of the surgeon, and if to the surgeon, why not to the physician and the accoucheur?

Sherbrooke, March 20, 1847.

ART. V.—REPLY TO PROFESSOR CROFT'S "CRITICAL REMARKS."

By E. S. DEROTTERMUND, Esquire.

(To the Editor of the British American Journal.)

SIR:—I trust Mr. Croft will understand distinctly, that if I have been unwilling to notice his analyses of the Brantford waters, or to bestow any serious attention

upon them, I was not deterred by any fear of entering into a scientific discussion; but simply because I observed in them errors of such magnitude, and so extraordinary, that I was disposed to regard his articles rather as the pretensions of a man desirous of exhibiting his ability to read and sign his name, than as the efforts of one capable of producing original matter, or even of copying correctly. Since, however, Mr. Croft, with significant pomposity, assails me, under his title of "Professor of King's College," I am induced, through respect for the situation he holds, to reply to his strictures. I make this observation, because I should never have considered myself obliged to notice him as a chemist; for this plain reason, that neither by his style or matter does he prove to me his right to that distinction; but most particularly the reverse by his last article, dated January 9th, and published in your March number.

Had Mr. Croft stated in his article, published in the number of your journal for June, 1846, that he had read the description of a mineral spring analysed by Mr. Bousingault, he would have acted more judiciously than to have re-produced that analysis so incompletely. Were not the distinguished merits of Mr. Bousingault so well and so extensively known, Mr. Croft would expose him to the reproach of being deficient in the most essential details, and those which are pre-eminently necessary in the publication of a quantitative analysis, in order that its value may be accurately appreciated. But I suspect that Mr. Croft, in his wish to imitate Mr. Bousingault, was not aware of what was necessary to render his article complete. What appears most unaccountable is, that one who styles himself "Professor of King's College," should commit a blunder so gross in regard to the use of carbonate of ammonia in determining the presence of magnesia, as would be unpardonable in a mere student of medicine. As Mr. Croft seems to be acquainted with some scientific printed authorities, if he had referred to some of those works for the use of tyros, who are receiving instruction in the means of ascertaining different bodies by this or that re-agent, he would have discovered his error.

Phosphate of soda forms, with magnesia, a double insoluble salt of phosphate of soda, and of magnesia in a neutral solution. In acid solutions, ammonia is added to neutralise the acid, and thereby to facilitate the formation of the double phosphate. Professor Croft employs carbonate of ammonia instead of caustic ammonia. If the solution be acid, it is certain that the carbonate of ammonia will be decomposed, forming a new salt, with an ammonia basis; and the carbonic acid, which becomes free, will form insoluble carbonic

salts, such as the carbonate of lime, of magnesia, of alumina, and the phosphate of soda can no longer act. Mr. Croft has, therefore, erroneously taken the carbonate of lime, or some other carbonate, for the phosphate of soda and magnesia. If Mr. Croft be a man of candor, he must admit that such a blunder is unpardonable, even in a Tyro, who recites his first lessons, and much more inexcusable in a "professor," who blunders into an analysis, thus—"ammonia precipitates a reddish brown flocculent substance, sesquioxide of iron, and perhaps alumina." For amusement, if not for edification, I should like Mr. Croft to inform us what he means by the science of chemistry, and what is the use or object of an analysis, if the chemist is to be as easily satisfied as the learned professor. "Sesquioxide of iron, and perhaps alumina," "little or no chlorine," "little or no protoxide of iron," and so on! This is precisely a case in which it may be safely said, that Mr. Croft is either a poor chemist, or, "perhaps," no chemist at all. He should have known that carbonate of ammonia yields a white precipitate, with salts of magnesia, barites, strontian, manganese, alumina, zinc, antimony, lead, tin, &c. Phosphate of soda yields a white precipitate, with salts of magnesia, lithia, barites, alumina, iron, zinc, antimony, tin, lead, &c. Now, what right has Mr. Croft to assume, or to take upon himself to state, that because he obtained a white precipitate by means of phosphate of soda (having previously added the carbonate of ammonia) he discovered the presence of magnesia, when these two re-agents yield a white precipitate, with so many others? Who knows but Mr. Croft, instead of magnesia, saw *lithia*, or alumina, or antimony, or zinc, &c. &c.?

Mr. Croft states the quantity of the different substances, and supposes, since Mr. Bousingault found sulphuric acid, hydrochloric acid, alumina, and lime in the waters near Papayon; and he, Mr. Croft, finding sulphate of sesquioxide of iron, lime, and magnesia in those of Brantford, that therefore the bed of this spring, like that of Papayon, must be volcanic. What absurdity! Every man of common-sense, not wishing to inflict on Mr. Croft the pain of contradiction, can only be amused at seeing him hunt for an analogy between the two analyses. A compassionate shrug of the shoulders is the only argument to be made use of in such cases. But let us come to his quantitative analysis.

First, Mr. Croft should have stated the quantity of the residuum, obtained from the evaporation of a given quantity of the water, say a pint, to prove if he made any losses, and in what stage of his analysis.

In June, 1846, Mr. Croft says he found sulphuric acid, sesquioxide of iron, magnesia, and lime. In his

communication of the month of August, he expresses his surprise at the presence of antimony in the waters of Brantford, and cites the analyses of Daubeny, Bischoff, and Ozann, to prove that that body does not exist in any mineral spring. We must presume, therefore, that the Professor denies the possibility of discoveries in chemistry, more particularly in mineral springs. We are justified in this belief, since the main ground upon which he rests his assertions is, that chemists, since the creation of the world, have not mentioned this body in any analysis of mineral waters.

In March, 1847, after Mr. Croft had perused my report, he finds, in the same mineral water, chlorine, protoxide of iron, soda, potassa, *alumina*, and "he adds that I do not mention any trace of free sulphuric acid, discovered and published by him" in June, 1840. I would ask him what process he adopted to detect its existence—more particularly, I would request him to inform me what course he took to convince himself that the sulphuric acid is or is not free? I suspect he will experience very serious difficulty to answer me these questions. I should believe him upon his word; and, as Mr. Croft publishes exact numbers, we must suppose that he weighed each of these bodies in a state of perfect purity. Our entertaining any doubts upon this important point would be a direct attack upon the honour of a chemist. So much the worse for Mr. C., if he has ignorantly or incautiously ventured to substitute protoxide for sesquioxide, sulphuric acid for chlorine, phosphate of soda and magnesia for carbonate of magnesia carbonate of alumina, of lime, &c. &c. Mr. Croft is his own accuser, and leads us to doubt the accuracy of what he advances so lightly and with such inconsiderate precipitation.

Mr. Croft, with affected irony, reproaches me with not having positively stated that the sulphuric acid is or is not free. I must admit that I am not provided with that audacity, or blind confidence in myself, which could alone have enabled me to bring before the public as a positive fact, what, in my opinion, could be given merely as a supposition. Whatever Mr. Croft's way of thinking or acting may be, mine is, that in scientific matters, one should rather be the notary than the barrister.

To state that sulphuric acid is, or is not free, requires a quantitative analysis, the most delicate and exact that can possibly be made, for supposing that there exists a certain quantity of acid *a*, and a certain quantity of base *b*, which would saturate but half the quantity of acid *a*, if now in analyzing a certain quantity of any substance, there is found a certain quantity of acid *a*, and if, instead of the quantity of base *b*, another quality and quantity is given without exactitude, can any one determine what is the

quantity of free acid present; for protoxide is not peroxide, nor is iron, alumina, each of those bodies having different powers of saturation with the same acid. And can Mr. Croft state by what rule or theory, approved by science, he has presumed to say, that the acid is, or is not free; but an answer to every one of Mr. Croft's accusations, would be a greater task than that which a professor might find with a tyro who has taken a wrong notion in his head. Mr. Croft misconstrues the thoughts, and the facts also; moreover, with a sort of self-satisfaction, he argues on a word, instead of the idea involved in the sentence.

Mr. Croft takes up my theory of the composition of the spring, in which I suppose that to have free sulphuric acid, the sulphate of lime must have been decomposed, which, with other circumstances, tended to facilitate the explanation, why antimony was in a state of solution—without saying if the sulphuric acid had entirely or partly served towards the formation of other salts. Mr. Croft seems to have considered this theory in the light of an analysis, as if I could have been *under the earth*, or have judged by any other means than those which are allowed to the disciples of science, viz., their intelligence based upon scientific information. If Mr. Croft is more capable, he can propound another theory. Many phenomena were at one time explained by the phlogistic theory, which has given place to the one based upon the action of oxygen. Neither theory, however, prevents mercury from becoming oxidated, by contact with air.

Mr. Croft, in June, 1846, states that the nitrate of silver did not detect anything in the waters of Brantford. He is mistaken; as there must have been formed a white precipitate of sulphate of silver. In March, 1847, he finds a white precipitate with nitrate of silver, and he takes it for chloride. He would have done better to inform us what means he adopted to separate sulphuric acid from the chlorine, in order to be certain that the precipitate obtained by the sulphate of silver was chloride and not sulphate of silver. Mr. Croft has had the goodness to admit, after reading my analysis, that he found some traces of chlorine. I thank him, also, for having found potassa, *alumina*, and soda. But this I regard as mere civility on his part, because he must permit me to doubt, if he saw these bodies by the method which he employed. He saw the yellow flame of the spirits of wine, but it was produced by organic matter, or by some other salt than pure soda, because he did not obtain it in a state of purity to render that test unexceptionable. Who knows that Mr. Croft may not also have mistaken a sulphate, phosphate, bromide, or iodide, &c., for the chloride, when the solution was tested by the nitrate of silver; inasmuch

as all these salts yield the same white precipitate with the nitrate of silver.

Thus, what opinion can or ought to be formed of a chemist who, after publishing three quantitative analyses of the same mineral water in June and August, 1846, and in March, 1847, at last finds six additional bodies, and all different from the first, which he supposed to have existed exclusively there? What ought we to say of a chemist who discovers a resemblance between two analyses, in consequence of the presence of a body which exists in nearly all the most common substances? Mr. Croft, in June, 1846, publishes different quantitative analyses, in which he states he found sulphuric acid. In the

1st Experiment,	21.670	grs.
2d do.	23.597	"
3d do.	22.049	"

without stating the quantities of matter upon which he operated. He only assures us that he found 7.680 grs. in one pint of water. In the month of March, 1847, he apologises, admitting that he was mistaken, and declaring that he had not had the time to make a complete analysis, and that, in fact, he did not possess the necessary materials. If such were the case, he ought to have acted with the prudence and integrity of a man of science, and not advanced and published what he could not sustain, much less prove. Mr. Croft supposes the soil of Brantford, where the spring is situated, to be volcanic; and, singularly enough, it is the presence of sulphates of an acid character which leads him to this conclusion. If Mr. Bousingault has declared that the bed of the spring near Papayon is volcanic, his opinion rests upon the fact that he found hydrochloric acid—hydrochloric and sulphurous acid may be indications of volcanos, but sulphates are not.

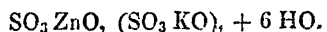
After criticising with so little gentlemanlike courtesy, after having so slipantly remarked upon the labours of another chemist, acknowledged as such by authorities in science possessing rather more celebrity in the scientific world than Mr. Croft, notwithstanding his title of professor—after having bedaubed and exposed himself to ridicule, in his own ambitious but utterly worthless publications, and that by his own showing, I put it fairly to Mr. Croft whether or not he is entitled to any further reply? I would add, that were I sure that he had the intention or the will to persevere in the study of chemistry, I should earnestly advise him to continue his investigations of the Brantford waters. This spring, or in 1848 at the very latest, he will hit upon some new ingredient, which has hitherto escaped his triple and thrice contradictory analyses.

You say with truth that my report was not sanctioned

by the signature of Mr. Logan. I may remark, however, that Mr. Logan, not being a chemist, could not in any way sanction that report. Be that as it may, I am most certain that that eminent geologist, whom you justly regard as a savant of high ability, would not consent to sanction Mr. Croft's analysis, much less would he uphold the supposition that the soil of Brantford is volcanic, because it contains sulphates of iron, of lime, and of magnesia.

I should have here terminated my reply to Mr. Croft, but being assailed by the *Argus*, a political, literary, and scientific journal of Toronto, which so loudly applauded Mr. Croft's article against me, I feel myself bound, through respect for the press, to offer a few more observations. As I have already said in my report, and I repeat it again, I could not give to the public any quantitative analyses; for notwithstanding every care in my investigation, I had not the necessary means of making them correctly; and as I regard the word of a man of science when he advances any fact connected with his labours as most sacred, I preferred publishing the qualitative analyses only, and to keep in my own possession the quantitative; because, although they appeared to be correct, they might have been inexact, for want of the necessary apparatus.

Mr. Croft expresses his astonishment at my finding zinc and potassa in solution in the water. He makes use of the printed signs of admiration, which so well depict his thoughts. If Mr. Croft does not know the existence of double salts of sulphate of zinc and of potassa, yet as a professor he should recall to mind that the salts known under the name of halhydrates, form double salts only with salts in which the acid has lost its water. If, having the wish to attack the labours of chemists, he knew a little of the science, he would have placed the exclamation thus, zinc and lime! I would have had the trouble only to object to him that he is mistaken in his reproductions; for he would be right in saying, that sulphate of zinc cannot form a double salt with the sulphate of lime, inasmuch as both are halhydrates; but the sulphate of zinc forms a double salt with the sulphate of potassa and others not halhydrate. The sulphate of lime will form them with the same salts. And here is the formula, according to Liebig, which perhaps will serve to calm Mr. Croft:



Other chemists give seven atoms of water.

As to the antimony, I obtained it in the state of sulphuret, of an orange red colour, giving with the blow pipe a white smoke, depositing itself on the coal in a circle. And to convince Mr. Croft that I take great pre-

cautions in my analysis, I would remark, that, fearful lest the presence of the antimony might have been due to the hydro-sulphuric acid, which might have contained some traces of that metal, derived from the material from which it was obtained, I expressly prepared some sulphuret of iron myself; and when I again obtained the orange coloured precipitate, I was convinced that antimony was contained in the solution. I am free to confess the extent of my surprise at this discovery; for I was aware that that body had not been previously detected in mineral springs. But Mr. Croft would never be able to find it, even if I were to point out to him the method of doing so, inasmuch as practical skill is necessary.

As to the proof of vegetable albumen, let Mr. Croft read the analysis of the waters of Barege, and afterwards examine the waters of Caledonia, if perchance another fit of chemical analysis should seize him. He will, no doubt, suppose that there are broken eggs in the soil, if he should discover vegetable albumen. As to the solubility of resinous matter in water, Mr. Croft is perfectly correct in his position. It is insoluble, as iron is, as also zinc and other bodies, and which nevertheless we find in solution in many springs. But since I am compelled to adopt language more practical than scientific, in order to be more intelligible to persons unacquainted with the science, I would mention certain daily practices, adopted by manufacturers to increase the weight of their articles, to the prejudice of the public. They mix resin with potassa, which, in the form of soap, is soluble in water. Analyse such a water, and you will not be surprised to find resinous matter in it in a state of solution. Since the manufacturer, in order to carry on his trade more successfully in a commercial point of view, has found out the means of overcoming or concealing the insoluble nature of resin, why should we doubt that nature, so mysterious in her existence, and frequently so in her operations which often seem incompatible to ordinary intelligences, might not also form some combinations, which, we can reasonably suppose, would escape Mr. Croft's scientific attainments. Mr. Croft finishes his article thus: "Mr. DeRottermund cannot expect that any one of his statements will be believed by the scientific portion of the people of Canada, although it is possible that so flourishing a report may obtain credence among those who may have as inaccurate a knowledge of chemistry as even Mr. DeRottermund himself, a portion of the community which, it is to be hoped, for the credit of the country, will be found to be exceedingly small." A direct answer to this is scarcely necessary, and would be of very little importance; but I will merely take the liberty of submitting a problem which can be solved by the latter

"portion of the community" just as well as by the "scientific portion of the people of Canada"—could not the combinations produced by nature be as well concealed from Mr. Croft's understanding, as are the preparations of an able artisan from the ordinary purchaser?

I prefer saying nothing to refute the assertion, that we can expect more in our laboratories than nature in her combinations; because the apothecary, who has the recipe for a preparation, may defy us to prepare his ointment. His skill in turning the pestle produces that which a chemist the most expert in the theory of combinations, could not imitate, and nature possesses far greater means and secrets of preparation than the most skilful chemist. The only correction which I expect to receive in regard to my labours, is, that I may have been deceived in the state of the salts. But if that error does exist, it does not arise from any want of exactness, but from the want of those necessary means which no chemist engaged in delicate analyses, can dispense with. In the beginning I declared frankly, that I was not possessed of what was necessary. In order to determine the state of the salts, an analysis more accurate than Mr. Croft could make would be required.

Errors frequently arise from the calculations and the figures arising out of the theory of the laws of combination. Thus a man corrects himself if he sees that another chemist has seized the truth with more accuracy than he has; but on that account a man is not less a chemist. Thus M. Liebig corrects M. Dumas, and M. Dumas corrects M. Liebig. And it is immaterial what my labours are, and be their merits what they may, I defy Mr. Croft to make that analysis, without my showing him at every step errors in principle, and inaccuracies; and I will prove to him that he has mistaken water for an acid, protoxide for peroxide, paper for volcanic matter, and who knows but some other substances, similar to those of the sour spring near Papayon, and others again still more remote, with which Mr. Croft would rejoice to compare them. But to bring this communication to a close, take care, Mr. Professor, not to do "as the facetious old gentleman did, who, hearing a friend cry out '*lapsus lingua*,' when his servant let fall a boiled tongue, caused his own attendant to be equally awkward with a round of beef, and expected to gain great applause by a repetition of the witticism;" the pretty story which you cite in your admirable critique, in the March number of the *British American Journal of Medical and Physical Science*, in answer to my labours.

St. Cesaire, March 30, 1847.

*Mesmerism in India, and its Practical Application in Surgery and Medicine.* By JAMES ESDAILE, M.D., Civil Assistant Surgeon, H. C. S., Bengal. London, 1846. 12mo. pp. 287.

The subject of Mesmerism is presented in the work, the title of which we have just given, under a garb in which it has never been previously invested. In reading its pages, we are forcibly reminded that the volume was not exclusively intended for the Profession, which, to a certain extent, detracts from the position which, otherwise, it might have occupied; but we are, nevertheless, not the less under an obligation to examine its contents, and to draw from them such conclusions as they appear legitimately to warrant.

The term Mesmerism has become with many minds so intimately associated with the idea of imposture—and this, too, not without ample grounds—that it is frequently impossible to obtain for it a patient investigation of those circumstances, which are announced under its name as *facts*. The means of disproof are very frequently not so ready of access, and minds impatient of results, usually discard the statement, or the *fact*, as it is usually termed, without due and proper inquiry. We believe this to be true in many of the instances of the induction of Mesmeric conditions, and while we wish to be considered as by no means yielding our assent to the truthfulness of every thing supposed to have been effected through alleged Mesmeric means, of which we may have either read or heard, or which we may have seen, we do yet believe that, in some instances, effects have been realized which should not have been overlooked by the Profession, but ought, on the contrary, to have elicited investigation in a more particular manner than has yet taken place, and which, had they been legitimately followed up, might have been productive of much good.

In entering upon a new field of inquiry, every advancing step should be marked with caution, especially when dealing with a subject like the present. We have, therefore, in estimating the value of the publication before us, to apply to it the ordinary rules of evidence. We have then to consider, in the first place, the credibility of the witness; and, in the second, the degree or amount of it which should attach to his statements: for, in a case like the present, we must not be insensible to the contingency, that the witness may conscientiously believe in the full truthfulness of his narrative, and overlook many circumstances, which to his mind, already prepossessed, may seem quite conclusive, yet when scrutinized rigidly, will not stand investigation. Now, as far as the first point is concerned, we cannot permit ourselves to believe, that Dr. Esdaile would, on a point in which detection, exposure, and certain disgrace would so quickly have succeeded imposition, have willingly and knowingly palmed off upon his Professional brethren a deception so gross, as what the present publication in that case would afford evidence of. His professional rank, his position in the civil service of the East India Company, in charge of an important public charity, all forbid the entertaining of any such idea. The motive for the publication of the work, is

one, moreover, which is of itself conclusive on this part of our subject. We give it in the author's words:—

"Mesmerism often comes to my aid when all the resources of medicine are exhausted, and all the drugs of Arabia are useless. I consider it my duty to benefit them (my patients) by it, and to assist in making it known for the benefit of mankind." And in another place he observes, "The public, in examining a subject so deeply interesting to them, will, I hope, take an enlarged and liberal view of the matter, and look for fundamental and incontrovertible truths, which are practically important, and not allow themselves to be cheated out of their senses and judgment, by the doubtful mysteries and theoretical parts of the subject being exclusively dwelt upon by those who wish Mesmerism to be untrue, or by others who have neither the desire nor the capacity to acquire new knowledge. Errors of observation and of judgment must often occur in investigating a new and difficult subject, but I hope such unintentional mistakes will be excused, and wherever they shall be pointed out in my observations, I shall be ready to acknowledge and correct them. Let all doubtful evidence be totally rejected, and a mass of substantial, important truth will remain, which I am confident the public will not let willingly die; for human nature can ill afford to lose any new and promising source of comfort to suffering humanity."

The motive for the publication being thus, according to the author's own shewing, based on the truest philanthropy, we are in an especial manner called upon to examine into the merits of the case submitted; for if the credibility of the witness be good, his motive philanthropic, and his statements unimpeachable, the Profession is bound, not only not to reject the evidence, but to examine the subject for themselves,—and this leads, in the second place, to consider the credibility of the statements.

Dr. Esdaile does not appear to have been entirely sceptical of the truth of Mesmerism. After having heard and read much about it, he came to the conclusion, that "if the twentieth part of what was reported was true, it well deserved careful investigation," and "I determined to try and find out the truth for myself." And as his first experiment is the one which led to the prosecution of his further inquiries, and the formation of his opinions, we cannot do better than detail the circumstances in the author's own words.

In choosing a proper subject to experiment upon, I should probably have selected some highly sensitive female of a nervous temperament, and excitable imagination, who desired to submit to the supposed influence. But, I beg it to be particularly remarked, my first essay was not guided by theory, and was not made on a subject supposed to be favourable. On the contrary, the very worst specimen of humanity, theoretically considered, was the person destined to be my first mesmeric victim; he being none other than a Hindoo felon of the hangman cast, condemned to labour on the roads, in irons. Accident alone determined my choice, and decided the matter for me, perhaps much better than theory would have done; for I should as soon have thought of commencing operations on the first dog or pig I met on the road, as of selecting this man for his good mesmeric "material."

There are some interesting particulars in this first successful mesmeric experiment in India, to which I beg leave to direct the reader's attention.

I. The purely accidental and unpremeditated nature of the experiment.

II. All want of consent between the parties.

III. The operator's want of belief in his own power; for I had never seen Mesmerism, and all I knew about it was from scraps in the newspapers.

IV. The absolute ignorance of the patient; it being impossible that he should ever have heard of Mesmerism.

V. The impossibility, therefore, of imitating the mesmeric phenomena.

Under all the circumstances of the case, collusion between the



parties will not, I presume, be suspected: and every possible care was taken to exclude any source of fallacy in the experiment. European gentlemen, sceptical and critical, or so strong in disbelief that they would have reasoned themselves out of their senses, if they could; ignorant Hindoos and Mussulmans, who merely used their eyes and ears without an attempt at reflection, will all be found, by their separate and independent reports, bearing testimony to the same series of phenomena. As I might never succeed again, I endeavoured to make this case as perfect as possible in all its parts, by bringing the senses of different people to bear upon it, in all its stages; and, I must say, that I cannot see any possible opening for mistake or deception. It has for many months been before the Indian public, who were invited to point out any source of error that may have escaped me, and no attempt has been made to disprove the facts, or explain them away, except by the easy and sweeping charge of imposition in the patient, and delusion in the observers; of the probability of which I shall now give the reader the means of judging.

#### First Experiment.

Madhab Kaura, a hog-dealer, condemned to seven years' imprisonment, with labour on the roads, in irons, for wounding a man so as to endanger his life, has got a double Hydrocele. He was ordered to be taken from the jail to the charity hospital, to be operated upon.

April 4th.—The water was drawn off one side of the scrotum, and two drachms of the usual cor. sub. injection were thrown in. On feeling the pain from the injection, he threw his head over the back of the chair, and pressed his hands along the course of the spermatic cords, closing his eyelids firmly, and making the grimaces of a man in pain. Seeing him suffering in this way, I turned to the native sub-assistant surgeon, an *élève* of the medical college, and asked him if he had ever seen Mesmerism? He said, that he had seen it tried at the medical college, but without effect. Upon which I remarked—"I have a great mind to try it on this man, but as I never saw it practised, and know it only from reading, I shall probably not succeed."—The man continuing in the position described, I placed his knees between mine, and began to pass my hands slowly over his face, at the distance of an inch, and carried them down to the pit of his stomach. This was continued for half an hour before he was spoken to, and when questioned at the end of this time his answers were quite sensible and coherent.

He was ordered to remain quiet, and the passes were continued for a quarter of an hour longer—still no sensible effect. Being now tired (thermometer 85°), I gave it up in despair, and declared it to be a failure. While I rested myself, the man remained quiet, and made fewer grimaces, and when ordered to open his eyes, he said there was a smoke in the room. This roused my attention, and tempted me to persevere. I now breathed on his head, and carried my hands from the back of his head over his face, and down to the Epigastrium, where I pressed them united. The first time this was done, he took his hands off his groins and pressed them both firmly down upon mine; drew a long breath, and said, "I was his father and mother, and had given him life again." The same process was persevered in, and in about an hour he began to gape, said he must sleep, that his senses were gone; and his replies became incoherent. He opened his eyes, when ordered, but said he only saw smoke, and could distinguish no one; his eyes were quite lustreless, and the lids were opened heavily. All appearance of pain now disappeared; his hands were crossed on his breast, instead of being pressed on the groins, and his countenance showed the most perfect repose. He now took no notice of our questions, and I called loudly on him by name without attracting any notice.

I now pinched him, without disturbing him, and then asking for a pin in English, I desired my assistant to watch him narrowly, and drove it into the small of his back; it produced no effect whatever; and my assistant repeated it at intervals in different places as uselessly. His back had continued to arch more backwards latterly, and he now was in a state of "opisthotonos;" the nape of his neck resting on the sharp back of the chair, and his breech on the edge of it. Being now satisfied that we had got something extraordinary, I went over to the Kutcherry, and begged Mr. Russell, the judge, and Mr. Money, the collector, to come and see what had been done, as I wanted the presence of intelligent witnesses in what remained to do. We found him in the position I had left him in, and he hallooing in his ears could at-

tract his attention. Fire was then applied to his knee, without his shrinking in the least; and liquor ammonia, that brought tears into our eyes in a moment, was inhaled for some minutes without causing an eyelid to quiver. This seemed to have revived him a little, as he moved his head shortly afterwards, and I asked him if he wanted to drink; he only gaped in reply, and I took the opportunity to give, slowly, a mixture of ammonia so strong that I could not bear to taste it: this he drank like milk, and gaped for more. As the "experimentum crucis," I lifted his head, and placed his face, which was directed to the ceiling all the time, in front of a full light; opened his eyes, one after the other, but without producing any effect upon the iris; his eyes were exactly an amaurotic person's, and all noticed their lack-lustre appearance. We were all now convinced that total insensibility of all the senses existed, and I ordered him to be placed on a mattress on the floor, and not to be disturbed till I returned. It was now one o'clock, the process having commenced at 11 a. m.

I returned at three o'clock, and was vexed to find that he had awoken, and been carried back to the jail hospital. The native doctor of the jail had come in; and on hearing that the Sahibs could not awake the patient, he set about doing so, and succeeded by throwing water on his face, &c. I again went to Messrs. Russell and Money, and requested them to accompany me to the jail, to be present when he was interrogated regarding his reminiscences; and we put down a series of questions to be put to him, at once, and without explanation. We found him looking well, with a lively expression of face, and the following questions were put to him; his answers being taken down at the same time:—

- "How do you feel?"
- "Very well."
- "Any pain in the throat, or elsewhere?"
- "A little uneasiness in the throat, but no pain any where else."
- "What has happened to you to-day?"
- "I went in the morning to the Imbarah Hospital, to get the water taken out of my scrotum."
- "Was the water drawn off?"
- "Yes."
- "What do you remember after the operation?"
- "I went to sleep soon after, and remember nothing else."
- "Did you eat or drink after the operation?"
- "I felt thirsty, but got nothing to drink till Kurroon Ali, the native doctor, awoke me."
- "Did any body prick, or burn you?"
- "No, no."
- "Did you smell anything disagreeable?"
- "No."
- "Were you happy when asleep?"
- "Very."
- "Did you hear anything when you were asleep?"
- "I heard voices, but did not understand them."
- "Did you see any gentleman in the hospital but me?"
- "No."
- "Did you feel any pain in the scrotum after going to sleep?"
- "I felt none till I awoke."
- "Any pain in that part now?"
- "A very little."
- "How many motions have you had to-day?" (he was suffering from chronic diarrhoea.)

"Four, before going to the hospital, none since; belly is much easier than it has been for some time."

Having answered all these questions readily and frankly, he began to cry, thinking it was some kind of judicial investigation, I suppose.

"The above is an exact relation of what took place in our presence, and we are thoroughly convinced that there was a complete suspension of sensibility to external impressions of the most painful kind.

(Signed,) F. W. RUSSELL.

D. J. MONEY.

BUDDEN CHUNDER CHOWDAREE.

Sub-Assistant Surgeon."

April 5th.—There is less than the usual inflammation, and he makes no complaint. I intend to operate on the other side in a few days, mesmerising him first, if possible, and have invited many persons to be present.



*Second Experiment.*

April 6th.—11 o'clock, a. m.—The inflammation has become high during last night; the part is hot, and excessively tender; the lightest touch causes great pain. Skin hot; pulse quick. I could not resist the temptation to satisfy myself still further, and relieve him at the same time. So, turning to the native doctors, I said that I would again try the "Belatee Muntur" (the Europe charm), and began the process as before, he lying in bed. In ten minutes the mesmeric haze (smoke he always calls it) was produced. After half an hour he still complained of the pain in the inflamed part, and could not bear its being touched; in three-quarters of an hour the coma was established, and I squeezed the inflamed part with no more effect than if it had been a bladder. Having business to attend to in Chandernagore, six miles off, I called, in passing, on the Rev. Mr. Fisher, and said that he might now satisfy himself by going to the hospital in my absence; and that, except mesmeric means, he was at liberty to use every possible means to awake him, or make him feel. Here I have the pleasure to introduce a report of the proceedings of Mr. Fisher and Mr. Money while I was at Chandernagore:—

"To J. Esdaile, Esq.

My dear Sir,—I beg to certify that I twice saw the native whom you had put into a mesmeric trance, or state of catalepsy, and from the successful application of different tests, I have no hesitation in stating, should my statement add any weight to your own testimony, or be of any service to the cause of the imperfectly known, and hitherto unfairly treated, science of Mesmerism, that the individual in question was in that state entirely insensible of pain, and that I believe, if you had cut his leg off, he would not have felt it. I saw, when I was in England, both publicly and in private, many cases of Mesmerism accompanied by unnatural and wonderful phenomena, without being convinced. But your case is one so free from all possibility of suspicion, that to have doubted it, one might as well have doubted his own existence.

"Yours truly,

"D. J. MOWER.

"Hooghly, 9th April, 1845."

"I have only to add to the above, that I was present upon the last occasion referred to by Mr. Money, and fully concur with him in thinking that the patient, during the Mesmeric trance, was totally insensible to pain. Indeed, all the senses appeared to be unnaturally suspended from any manifestation of their ordinary operations, and every available test was tried in vain. Dr. Esdaile upon this occasion was absent at Chandernagore, having previously put the patient into the trance.

"F. FISHER."

Returned to the hospital at three o'clock, and found him lying just as I had left him. Awoke him in a few minutes, by rapid transverse passes, blowing in his face, and giving him water to drink. Is free of pain, and still desires to sleep; says his head turns.

*Translation of a Report from Kurreeem Ali Khan, native doctor, of what he saw and heard in the Jail Hospital, on the 6th April, 1845.*

"At 11 o'clock, a. m., the patient, Madhab Kaura, was in a fever, and there was an acute pain in the scrotum. The worthy Dr. Sahib (may he ever prosper) came to the hospital, and began to do something to him. When the experiment was going on, Madhab was asked—

"What do you see?"

"I cannot see clearly; something like smoke is before my eyes."

"Do you see the doors?"

"No, nothing but smoke."

"Do you see Dr. Sahib?"

"No, I see nobody, but perceive some one is talking near me."

"Is there any pain in your body?"

"Yes, breathing causes pain in the belly."

"Is there pain in the scrotum?"

"Yes, as acute as ever."

"How do you feel now?"

"I feel cold and sleepy."

After the Doctor Sahib had tried for nearly three-quarters of an hour, he fell into a deep sleep, and there seemed to be no pain in

the scrotum; he slept so sound that even the pricking of his body with a pin did not restore his senses, or awake him. Before, a touch of the scrotum was painful, but after he was asleep, even pricking it caused no pain whatever.

He continued in this state for three hours, when, the Dr. Sahib calling him aloud twice or thrice, he came to his senses, and opened his eyes. He asked for water, which he drank, and, feeling cold, covered himself."

*Translation of a Report from Nohoo, native doctor, of what he saw and heard in the Jail Hospital, on the 6th April, 1845.*

"On the morning of the 6th of April, I went to the hospital, and found the body of Madhab Kaura hot and feverish, and he felt a great pain in the scrotum

"At 11 o'clock, Dr. Esdaile, the civil doctor, came and made some operations on the body: something I do not know. While the operation was going on, the patient was asked if he could see plainly, but said no. When asked if he could see any one, he answered, that he could see no one, but knew by the sounds that some people were there.

"Again he was asked if he felt any pain; he said, that he felt a severe pain in the belly, on breathing, and also in the scrotum, and felt very cold. Soon afterwards he became senseless."

"At 2 o'clock p. m. the Rev. Mr. Fisher and Mr. Money came to the hospital, and tried to bring him to his senses by pricking him with a pin, putting fire on his hand, and beating a gong in his ear, but all proved ineffectual."

I forgot to note down what these reports notice;—his complaining of feeling cold soon after the process began; and that when I left him the temperature of his body was natural.

On these two occasions, the effects were witnessed by all the patients and hangers-on in and about both hospitals.

April 7th.—Has had a good night, is a little feverish; pain in scrotum much less. He now complains, for the first time, of pain in the places where he was pricked and burned.

This makes one ashamed of incredulity, and I will never put a patient to the "question" in this way again. It is only excusable for the first time, when we can hardly believe the evidence of our senses.

*Third Experiment.*

April 11th.—Took the sub-assistant surgeon with me to-day to the Jail Hospital, and desired him to watch the time taken to produce the different effects. There is still considerable pain in the side operated upon. Pulse regular, 60; skin warm. At 11 o'clock a. m. I seated him on the floor with his back against the wall, placed myself before him on a stool, and proceeded pretty much as before. The process, in one particular, was varied. I leaned my elbows upon my knees, placed my mouth over the back of my joined hands, and breathed along their upper surface; the points of my fingers being pointed steadily at his eyes, nose, and forehead, in succession. This seemed to be very effectual, and was done with the idea of concentrating the mesmeric influence of the whole body into one conductor. It was curious to observe that he had begun to think on the subject, and was observing the effects for himself, and testing his senses as we proceeded. After manipulating for a few minutes, he opened his eyes, looked sharply and minutely about him, and being asked if he saw quite well, he said,—“Oh, yes.” In a minute or two he repeated his inspection, and answered again, that he saw quite distinctly; in seven minutes he again looked about him, seemed surprised, and said he only saw “smoke.”

In fifteen minutes he was pinched; and when asked if any one was pinching him, he replied that he could not tell, as I might now cut a piece out of his body without his feeling it. I now tried for an abnormal mental manifestation; certainly not expecting to be gratified. I asked—

"What will cure your complaint?"

"You know best."

"Has the Baboo any complaint?"

"How should I know?"

I understood this as a hint to attend to the business in hand, the body, and therefore proceeded to induce the mesmeric coma as quickly as possible; and succeeded in twenty minutes from the commencement. I then said to the sub-assistant surgeon that I would operate upon him in this state, if I could find some of the European gentlemen to be witnesses. On going to Chinsurah, two miles off, I fortunately found a considerable party, consisting

of the Baron Law de Clapernon, Governor of Chandernagore. Mr. Russell, the judge, Mr. Waichope, the magistrate, J. St. Pourcain, Esq., Mr. Clint, Principal of Hooghly College, and Mr. Clermont, head master of the Lower School, who all accompanied me back to the hospital. The man had fallen down, and was lying on his back. The large gong of the jail was brought, and struck violently within a few inches of his ear, with no effect. I then pierced the scrotum, and threw in the injection, without any one being sensible of the smallest movement in his face or body. His limbs were quite flexible; but on holding one of his legs in my hand for a few seconds, it gradually became quite rigid, and we could not bend it again; the same occurred in the other leg. The arms were supple, and lay in any position into which they were thrown; and when the fore-arm was bent upon the humerus, and then let go, it fell upwards, or downwards, instantly. But on placing my united fingers over the ends of his, the arm remained fixed at a right angle in the air, and swayed to and fro, according to my movements. The insensibility of the iris was also tested, and proved.

Six o'clock a. m. Still sleeps; most complete relaxation of all the limbs now exists. The legs and arms can be tossed about in every direction, and where they fall there they lie. Being curious to ascertain the effect of the artificial state on the natural process of inflammation, I did not awake him, but saw that the part was as flaccid as when the water was just withdrawn.

April 12th.—He awoke at twelve o'clock last night, spontaneously. Recollects nothing after going to sleep; sees the water is gone, knows not how; supposes the Doctor Sahib did it. The testicle is considerably enlarged to-day; there is little pain, and it did not swell till after he awoke. He has had chronic diarrhoea for some time; four or five motions a day; but has had none since yesterday forenoon till this morning. Natural, artificial, and diseased actions have therefore been all equally arrested for the last thirteen hours; a practical fact of the utmost importance, which will not be lost sight of by myself, or others, I hope. What a blessed prospect this opens to sufferers who may be sensible to the Mesmeric influence! In time we may hope to discover who they are, by detecting the laws which regulate this power of Nature, and thereby save ourselves much trouble and disappointment. In the meantime let us accumulate *facts*, as the seed for a correct theory hereafter. Although I should never succeed again, I will, in future, think, speak, and write of Mesmerism as being as much a reality as the principle of gravitation, or the properties of opium. For, under all the circumstance, I cannot but consider these to be unexceptionable *facts*; and if I should not again be able to elicit them, it would not shake my belief in the existence of Mesmerism; I should only conclude that the failure arose from my ignorance of the conditions required by Nature, or from some personal disqualification. The rarity of the occurrence would not make it less a reality; and to deny a fact because it has been seldom witnessed, would be as reasonable as to doubt the existence of comets because they are rare appearances.

Great weight is very justly attached to *first experiments* in any new subject of investigation, for these are often a voluntary and unexpected evolution of the powers of nature; and when the results surprise the experimenter even, we feel confident that he only relates what he actually saw, and that he is not seduced, by previous theory and prepossession of mind, to interpret appearances in support of a foregone conclusion. In making these experiments, I was in the situation of a chemist, who has heard that a new elementary substance had been discovered by a certain process, and who thereupon sets his apparatus to work in the way prescribed, and is rewarded by obtaining the same results as the first discoverer.

Besides the general results, I noted in this case the following particulars as *facts*, which determined the course of my future proceedings.

I was sure that there could be no imagination at work in the matter.

That there was no consent between the parties.

No mental sympathy.

That the patient's eyes need not be open.

I therefore came to the conclusion, that in this instance, the influence must have been of a purely physical description, and on this supposition I conducted my subsequent experiments; with what success will be shortly seen.

Such then is the first case in which Dr. Esdaile operated

when the patient was in a state of Mesmeric coma, and the success of which is, as we have seen, amply attested. From this period to the 22nd January, 1846, a period of about eight months, he performed the following unexampled series of operations under similar circumstances:—

*A Return showing the number of painless Surgical Operations performed at Hooghly during the last eight months.*

Arm amputated, 1; Breast amputated, 1; Tumour extracted from the upper jaw, 1; Scirrhous testium extirpated, 2; Penis amputated, 2; Contracted knees straightened, 3; Contracted arms straightened, 3; Operations for cataract, 3; Large tumour in the groin cut off, 1; Operations for hydrocele, 7; Operations for dropsy, 2; Actual cautery applied to a sore, 1; Muriatic acid applied to a sore, 2; Unhealthy sores pared down, 7; Abscesses opened, 5; Sinus, six inches long, laid open, 1; Heel flayed, 1; End of thumb cut off, 1; Teeth extracted, 3; Gum cut away, 1; Prepuce cut off, 3; Piles cut off, 1; Great toe nails cut out by the roots, 5; Seton introduced from ankle to knee, 1; Large tumour on leg removed, 1; Scrotal tumours, weighing from eight pounds to eighty pounds, removed 17—painless, 14; Operations, 73.

In all the operations, however, of which we have record in the work before us, complete insensibility during the whole of the operation was not invariably observed. We are informed of three cases in which the insensibility was incomplete. But Dr. Esdaile observes, that imperfect though it was, it proved "a great comfort to the patient, and afforded great facilities to the operator." We adduce an example of this imperfect class.

April 23d.—Bachoo. Saw him for the first time to-day, at 11 o'clock, a. m.: he has got hypertrophy of the scrotum; the tumour is twice the size of a man's head. I put him to sleep, and made his arms cataleptic in three quarters of an hour.

Pricking and inhaling ammonia disturbed, but did not awake him; I therefore proceeded to operate, but he awoke after I had slit up the prepuce. Upon this I desisted, and will try to educate him into insensibility. To be mesmerised daily.

April 27th.—He has been easily mesmerised daily, since the 23d; is not insensible to pricking, but it does not awake him, and I could afford no more time to him. I pulled him by the legs to the end of the table; allowed the tumour to hang down unsupported, and bent his knees, putting his feet on the edge of the table; and in this painful attitude he remained for half an hour, without moving. His legs and arms were then properly disposed of, in case he should awake, and the tumour quickly removed; the first incisions did not awake him, but before I was done he was completely roused.

The following successful case of the removal of a scrotal tumour, the patient being in a state of complete insensibility, we record, as much in consequence of the enormous weight of the mass removed, as on account of its being a fair sample of the other cases of a like description which are detailed.

Oct. 25th.—Gooroochan Shah, a shopkeeper, aged forty. He has got a "monster tumour," which prevents him from moving; its great weight, and his having used it for a writing-desk for many years, has pressed it into its present shape. His pulse is weak, and his feet oedematous, which will make it very hazardous to attempt its removal; but with such an appendage life is literally a burden. He became insensible on the fourth day of mesmerising, and was drawn with the mattress to the end of the bed (my usual mode of proceeding): two men then held up the tumour in a sheet, pulling it forward at the same time, and, in the presence of Mr. Bennett, I removed it by a circular incision, expedition being his only safety. The rush of venous blood was great, but fortunately soon arrested; and, after tying the last vessel, the mattress was again pulled back upon the bed, with him upon it, and at this moment he awoke. The loss of blood had

been so great that he immediately fell into a fainting state, and it took a good while to remove him. On recovering he said that he awoke while the mattress was being pulled back, and that nothing had disturbed him. The tumour weighed eighty pounds, and is probably the largest ever removed from the human body. I think it extremely likely that if the circulation had been hurried by pain and struggling, or if the shock to the system had been increased by bodily and mental anguish, the man would have bled to death, or never have rallied from the effects of the operation. But the sudden loss of blood was all he had to contend against; and, though in so weak a condition, he has surmounted this, and gone on very well.

Dec. 1st.—Has been allowed to go home at his own request: the wound is filling up slowly, for want of integument.

The following, selected at random, will complete the series of cases, for which we have room, in which the insensibility to pain was equally complete.

Dec. 26th.—Goluck Seit, a prisoner, has got a hydrocele on each side. A young Hindoo subdued him to-day in ten minutes, on the first trial. When about to operate, I saw that he possessed a consecrated nail, on one of his little fingers; and knowing the value attached to this, I resolved to get possession of it, if possible, as a moral test of his being insensible, for he would as soon have cut a cow's throat and eaten a beefsteak as allowed me to cut off his nail, while in possession of his senses. It is a common practice with the Hindoos to vow their hair, beards, or nails, to Shiva, the Destroyer, in the hope of averting his anger; and this man had consecrated his little finger-nail to Shiva Forakissore.—Forakissore, in this district, being a famous shrine of the god. I transferred the sacred excrescence to my pocket, without any remonstrance being made, and then performed the less formidable operation of withdrawing the water, and throwing in the injection, of which he knew nothing, on awaking two hours after. His only distress was the loss of his nail, and he spent hours in hunting for it, supposing that it had been broken off by accident.

Dec 29th.—I entranced Goluck Seit to-day in five minutes; and in the presence of Mr. Cahnsac and Mr. Mullins, operated on the other hydrocele, to which he was as indifferent as on the first occasion. But before putting him to sleep, I showed the gentlemen how painful was the side operated on three days ago; and yet, in five minutes after he allowed me to squeeze his testicle to any extent, without exhibiting a vestige of uneasiness. I awoke him in half an hour, that Mr. Mullins might question him; and he said that he saw the water was gone, but how it had escaped he had no idea.

May 26th.—Ram Dass, a large robust man, has a supernumerary tooth between the eye-tooth, and the first grinder, growing horizontally into his mouth, and causing him great annoyance. I entranced him in a quarter of an hour, lying on a mattress on the table, and proceeded to open his jaws. It cost me some trouble to relax the temporal muscles, and I had to proceed cautiously, as he did not appear to be under the extreme influence of the mesmeric power. From its position, it was difficult to lay hold of the tooth, but it was at last grasped, and extracted. He moved, and moaned a little, but I soon tranquilized him again, and he did not awake till almost suffocated by the blood. He declared he awoke from this cause, and not from pain.

Dec. 21st.—Samoo, a weaver, has got a bad sloughing sore of the prepuce and glans, of a year's standing.—To be mesmerised.

Dec. 22d.—He was mesmerised for two hours yesterday, and slept an hour afterwards, apparently natural. To-day I saw him after half an hour's mesmerising, when the trance was fully established, his whole body being rigid. As I had not time then, I left him, and returned in an hour, and found his body still stiff. I cut open and took off the prepuce; and fusing the glans half eroded, I cut it off too. The man showed no sign of life; the body continued stiff, and the pulse natural. He awoke in half an hour afterwards, and did not discover that anything had been done to him till he went to make water.

May 5th.—Rantoonce Buttachangie, a Brahmin, aged forty. There is a prodigious Fungus hamatodes protruding from the left elbow-joint. A swelling took place at the joint when he was five years old, and has gone on increasing gradually, but the skin remained entire till an incision was made by a native doctor, twelve days ago, when the bloody mass started through the integuments.

It exactly resembles the contents of an old aneurism; the structure of the fungus having been broken up by the actual cautery applied to it all over, in order to stop the bleeding: it was a frightful mass. I desired him to be carefully mesmerised, and went to Chinsurah, to consult with Dr. Elton, in charge of the troops there. We returned to the hospital together, and found him in a profound sleep, and decided to take the arm off instantly. It was removed without his moving or complaining, and Dr. Elton assured me that his countenance had never changed. He awoke immediately after the limb was off, and declared, again and again, that he was aware of nothing having been done to him till he awoke and saw his arm was gone; and he then saw Dr. Elton for the first time.

May 13th.—Is doing well.

May 16th.—He complains of pain in the stump to-day.—To be mesmerised.

May 17th.—He was easily put to sleep yesterday, and slept for three hours; was free from pain when he awoke, and continues so.

Oct. 8th.—Nazir, a peasant, aged sixty, has suffered from enlarged and scirrhous testis for four years; the parts are as large as a child's head, and extirpation is necessary.—He was entranced after two hours to-day.

Oct. 10th.—He was mesmerised the second time, to-day, in the presence of Mr. Sutherland, Dr. Owen, the Rev. Mr. Bradbury, Major Riddell, Mr. Higgen, Mr. Muller, Mr. Graves, Messrs. Savigny, Mr. Calder, and Mr. Bartlett.

I removed the parts without his showing any sign of sensibility till the last artery was being tied: he then woke up, but went immediately to sleep again for half an hour, and on awaking, said that he was only conscious of a little pain when he awoke for a moment, and found me to be tying something. He was cheerful and talkative, and showed no signs of suffering or exhaustion in his countenance or manner, and said the pain in the wound was very trifling.

The question now naturally arises after a perusal of the cases which we have extracted, and which are fair samples of those reported, could Dr. Esdaile have been imposed upon, and if so, what was the object to be gained by so general an attempt of the kind. We require not to be informed of the cunning and deception which are notorious traits of the Hindoo character, more strikingly characteristic of this race, than, perhaps, of any other under the sun, nor need we be reminded (a feature of their character equally notorious.) of their physical endurance of pain, of which, in their religious rites and ceremonies, innumerable instances might be adduced. These points must not be forgotten. But it requires a credulity of no mean order to entertain the idea, that a consentaneous and general system of deception had been practised here, that all the patients upon whom these operations were performed—operations among the most severe in the annals of surgery—agreed to evince no sign of suffering or pain, and that they succeeded with such stoical indifference and apathy, that scarcely a muscle of their faces or their bodies quivered. Dr. Esdaile himself disposes of such objections in the following summary method.

Since then I have had every month more operations of this kind than take place in the native hospital in Calcutta in a year, and more than I had for the six years previous. There must be some reason for this, and I only see two ways of accounting for it: my patients, on returning home, either say to their friends similarly afflicted, "Wah! brother, what a soft man the doctor Sahib is! He cut me to pieces for twenty minutes, and I made him believe that I did not feel it. Isn't it a capital joke? Do go and play him the same trick; you have only to laugh in your elbow, and you will not feel the pain." Or they say to their brother sufferers,—“Look at me; I have got rid of my burthen, (of 20, 30, 40, 50, 60, or 80 lbs., as it may be,) am restored to the use of

my body, and can again work for my bread: this, I assure you, the doctor Sahib did when I was asleep, and I knew nothing about it;—you will be equally lucky, I dare say; and I advise you to go and try; you need not be cut if you feel it." Which of these hypotheses best explains the fact my readers will decide for themselves. It ought to be added, that most of these persons were not paupers, but people in comfortable circumstances, whom no inducement short of *painless* operations could tempt to enter a charity, or any other hospital; and all who know the natives are aware of this.

Although the cases are narrated in the author's own words, and many are given without attestation, yet, as a great many are attested by competent and sensible observers, and among these were medical gentlemen, some of whom were pre-possessed against the subject, we conceive that due credit may be safely awarded to those that are not. On the ordinary rules of evidence, then, we may observe, that Dr. Esdaile has made out a fair case, so far as his surgical practice is concerned, and that in the *generality* of the cases which he has detailed, insensibility to pain was successfully attained. But we wish not to found our conclusion on so broad a basis; if in a few cases, and a few only, and of this we think there can be no reasonable doubt, insensibility to pain, by means designated mesmeric, was produced, we have then sufficient evidence to demonstrate, that the attention of the profession ought to be directed to the subject; were it only on the ground, that means of alleviating pain in surgical operations is a matter of deep and serious moment, and well worthy of that philanthropy which is one of the brightest ornaments of the character of the medical profession.

So much then for the application of mesmerism to surgery, a part of the work of Dr. Esdaile, which we consider as furnishing by far the strongest arguments in its favour.

2. The following is a return of medical cases cured by mesmerism during the last eight months:—

*A Return of Medical Cases cured by Mesmerism, during the last eight months.*

Nervous headache—cured by one trance, 3; Tic-doloureux—cured by one trance, 1; Nervousness and lameness from rheumatism of two and a half years standing, 1 by chronic treatment\*; Spasmodic colic, 1 by one trance; Acute inflammation of the eye, 1 by repeated trances in twenty-four hours; Chronic inflammation of the eye, 1 by chronic treatment; Acute inflammation of testes, 1 by repeated trances in thirty-six hours; Convulsions, 1 by one trance; Lameness from rheumatism, 2 by chronic treatment; Lumbago, 1 by general and local mesmerising for a week; Sciatica, 1 by general and local mesmerising for a week; Pain in crural nerve, 1 by general and local mesmerising for a week; Palsy of one arm, 1 by general and local mesmerising for a month; Palsy of half the body, 1 by general and local mesmerising for six weeks; Feeling of insects crawling over the body, 1 by one trance. —Total, 18.

The evidence afforded us as to the really curative agency of mesmerism in all these cases, even upon Dr. Esdaile's own showing, is by no means clear and conclusive. We will cite a few of such cases.

May 7th.—Nazir, a Mussulman, aged twenty, is suffering from the *sequela* of ophthalmia of two months standing; the sight of the left eye is destroyed. The cornea of the right eye is muddy with superficial ulcerations, and a pterygium is forming; there is constant lacrymation, and he cannot distinguish a white man from a black. I placed him in a chair before me, and directed the

operation to the eyes and head generally, desiring him to mention what he felt as we proceeded. He soon said, that he felt an agreeable warmth where my fingers passed without touching him; shortly after, he said his eyes were easier, and on extending the process to the body, he felt a general warmth pervade it, and sweat stood in drops on his face. He next said, that he felt a fear come over him that he could not account for, and desired greatly to sleep: having no desire to go farther, I here stopped:—the eyes to be mesmerised daily for ten minutes, and then be put to sleep.

May 22d.—Has been mesmerised daily, and put to sleep twice: he always feels better after the process; says that his body feels pleasant and light, and the expression of his countenance is much improved. The lacrymation has ceased, and he read two words in Bengalee and Persian to-day; the pterygium will be the only impediment to his sight.

In this case the report ceases on the 22d, but without stating that the patient was discharged, although it observes he was improved in condition. We are not informed of any correlative treatment. Are we to suppose that none was employed? We think the patient, at the very least, would not have been allowed the uncontrolled freedom of the hospital, with exposure to bright day light at all times. But we find that he remained a fortnight in the hospital, at the expiration of which the minor symptoms had all subsided; thus allowing us to doubt, to no slight extent, how far nature alone operated to a successful issue. Again—

June 6th.—I was called at eight o'clock last night, to see the wife of Baboo Essanchander Ghosal, deputy-magistrate of Hooghly. I found her in dreadful convulsions; she was speechless, and suffering from a constriction in the throat, that threatened to suffocate her every minute; and she constantly beat, or pointed at the part. At one moment her body was perfectly rigid, and in another it was bent back like a bow, till she rested on the back of her head and heels only. I never saw such convulsions except in Tetanus and Hydrophobia, and all I knew of the resources of medicine was useless; for how could she take physic when she could not take breath? I therefore had recourse to my new solvent power, and, after nearly an hour's hard work, I left her asleep, and cataleptic.

July 1st.—She has had no return of the fit. This is the lady for whose relief the conjuror was sent, but came too late.

Here we have no difficulty in recognising a severe attack of hysteria, the paroxysm subsiding after an hour's duration; a by no means unusual occurrence. Several other cases are reported of a character equally as unsatisfactory as those quoted. We certainly think that Dr. Esdaile has, in many of his reported medical cases, fallen into the very common error, of sanguine minds, of associating the *post hoc* with the *propter hoc*. He saw what he desired to see, and he immediately connected the effect produced with the means employed.

As far as regards the application of mesmerism to medicine one general observation may be safely hazarded, and it is this, that if such means are really capable of producing peculiar effects on the system, and of this, the details involved in the surgical cases, which we have given at some length on a preceding page, afford no slender evidence, it is natural to anticipate that such influences would be most likely experienced by persons of a highly sensitive temperament, and prove most beneficial (if beneficial at all) in diseases in which the nervous system is principally engaged—or the neuroses. The medical cases above enumerated would tend to point to such an inference, for, with few exceptions, they may be all classed under this head, although, in many of them, the proof of beneficial results thus in-

\* By chronic treatment is meant daily mesmerising without the intention of entrancing the patient, which is not necessary.

duced rests upon evidence by no means satisfactory or conclusive.

The following extract will exhibit the method adopted by the author to elicit Mesmeric coma:—

*Coma*.—I usually procure in the following manner, and am inclined to think that its comparative rarity in Europe is owing to the mesmeric influence not being at once sufficiently concentrated on the patient, by transmitting it to his brain from all the organs of the operator, and through every channel by which it can be communicated. With the necessary degree of patience, and sustained attention, the following process is so effectual in producing coma, that in a large enough field, and with properly instructed assistants, it may here be obtained daily, for the purpose of procuring insensibility to surgical operations. No trial under an hour should be reckoned a fair one: two hours are better; and the most perfect success will often follow frequent failures, but insensibility is sometimes induced in a few minutes.

Desire the patient to lie down, and compose himself to sleep, taking care, if you wish to operate, that he does not know your intention: this object may be gained by saying it is only a trial; for fear and expectation are destructive to the physical impression required. Bring the crown of the patient's head to the end of the bed, and seat yourself so as to be able to bring your face into contact with his, and extend your hands to the pit of the stomach, when it is wished; make the room dark, enjoin quiet, and then shutting your patient's eyes, begin to pass both your hands in the shape of claws, slowly, within an inch of the surface, from the back of the head to the pit of the stomach, dwelling for several minutes over the eyes, nose, and mouth, and then passing down each side of the neck, go downwards to the pit of the stomach, keeping your hands suspended there for some time. Repeat this process steadily for a quarter of an hour, breathing gently on the head and eyes all the time. The longitudinal passes may then be advantageously terminated, by placing both hands gently, but firmly, on the pit of the stomach and sides;—the perspiration and saliva seem also to aid the effect on the system.

In the quotation which we have now given, it will be perceived, and the same thing is developed in various pages of the volume, that the author accounts for the Mesmeric phenomena, on the supposition of an influence emanating from the organs of the operator, and concentrated in the patient. The theory which is thus propounded, savours strongly of the wild and vague speculations of Mesmer, on the equally croneous, although more plausible, because more logically demonstrated, hypothesis of Townsend. In reasoning on the manner in which Mesmeric states are induced, we must observe, that no proof whatever has been yet afforded of such emanating influences; and we think, that Mr. Braid has approximated the most closely, to the probable reality, in assigning the phenomena to mental impressions in the first place, which, by strong concentration, secondarily alter or affect the balance of the circulation of the blood between the brain and spinal cord, and the extremities, determining it to the two former in abnormal proportion.

In conclusion, for we have now devoted more space to this article than was our original intention—the subject of Mesmerism is either true, or it is false. Undoubtedly many extravagancies have been perpetrated under its name: but is every thing which has been recorded of it extravagant, or is every thing equally so? The volume which we have thus critically examined, may make us pause before answering such a question affirmatively. It behoves the Profession now to examine the subject for themselves. If false let it be proved to be so; if true, no matter to how trifling an extent, let its therapeutic value,

to that extent, be determined. Dr. Esdaile, in summing up the results of his experience, thus remarks:—

“I beg to state, for the satisfaction of those who have not yet a practical knowledge of the subject, that I have seen no bad consequences whatever arise from persons having been operated on when in the Mesmeric trance;” and “less constitutional disturbance has followed than under ordinary circumstances;”—

a strong inducement to the prosecution of further experiments.

## PRACTICE OF MEDICINE AND PATHOLOGY.

*Treatment of Scrofula*.—Iodide of iron in syrup, four grs. in twenty-four hours, continued not longer than a fortnight or three weeks at a time, then give aperients, and resume the iodine. The hydriodate of potash may be given more freely. Chloride of barium is very useful in cases of tallow-like complexions, pale tongue, and languid circulation, with irritability of the mucous surfaces. Make a solution of one gr. to ℥j. distilled water, and ten drops of tinct. gent. c., then take half oz. twice a day, and increase the dose if necessary to three grains daily.

Hydrochlorate of lime, ℥j. to 3xx. aq. dist., and give a teaspoonful in milk two or three times a day. The dose may be increased to two teaspoonfuls. It, as well as the alkalis and burnt sponge, is of doubtful value.

Cod liver oil is useful by improving digestion and nutrition, rather than by the specific value of the iodine or bromine it may contain. (Mr. Phillips)

In scrofulous abscesses, white swelling, chronic eczema, goitre, ulcerated ganglia, herpes, lichen, ulcerated lupus, macule, ophthalmia (chronic) complicated with ulcerating keratitis, have received much benefit by treatment with the union triple compound of chlorine, iodine, and mercury. “Iodhydrargirite de chlorure mercurieux.” (M. Rochard.)—*Braithwaite's Retrospect*.

*Physical Signs of Incipient Phthisis*.—M. Dubini has communicated the results of his researches into this difficult subject in semeiology. His ideas are for the most part in accordance with those of Fournet, Jackson, Louis, and others, to which he gives valuable confirmation.

In order to study the true signification of modifications of the expiratory murmur, as a diagnostic sign in incipient tubercularization, M. Dubini first endeavours to form an exact appreciation of this murmur in a state of health. As regards its duration and intensity, he adopts the scale of Fournet, which makes it as two, the respiration being as ten, in preference to the evaluation of Barth and Roger. He also lays great stress on the observations of Louis, who found the expiratory murmur prolonged under the right clavicle, but never under the left, in seventeen females exempt from pulmonary disease.

Prolonged expiration is not exclusive! confined to the first stage of phthisis; it is met with in chlorosis, in pulmonary oedema, in severe heart diseases, in pleuritic effusions, in bronchitis, and in emphysema; but in emphysema, the expiration is whistling; in bronchitis, which is seldom partial, the whistling expiratory murmur is generally diffused over the chest; and so in other diseases in which the prolonged expiration is present, it exhibits certain peculiarities which distinguish it from the prolonged expiration due to tubercular deposit.

M. Dubini does not regard the above sign as constant in all varieties of tubercular deposit; it is absent when the matter is agglomerated in voluminous masses, (crude tuberc.) between which the pulmonary tissue remains crepitant. The variety in which it is commonly noticed is that which consists in a general infiltration of the pulmonary tissues with miliary granulations. It appears then that prolonged expiration may exist without tubercles, and tubercles without prolonged expiration; but there can be little fear of error when the expiratory bruit is persistent and rough, and more especially if it is unequal, interrupted, and limited to one or other subclavicular region. The diagnosis is rendered next to infallible, if, with this sign, there are accompanying general symptoms proper to the disease.—*Gazette Médicale*, No. 51, 1846.

*On the Use and Abuse of Mercurial Preparations*—Dr. Sichel gives the following cautions as necessary in the exhibition of mercurial preparations:—

1. *The diet* must be in no-wise stimulant, and as little nourishing as possible. If this is not attended to the plasticity of the blood becomes augmented.

2. All notable change of atmospherical temperature should be avoided. Unless this rule be observed, numerous disappointments will occur, and premature salivation is especially likely to be induced.

3. It is a general law that the *special physiological action, or the toxic effect of a medicinal substance, only manifests itself after its action upon the pathological condition has become exhausted.*

The operation of this law is well seen in the employment of narcotics in those affections of the nervous system which afford distinct indications for their use, as neuralgia and tetanus. This last, we know, demands large doses of opium, but the point of saturation must be carefully watched so that the drug may be laid aside when the precursors of narcotism begin to replace the tetanic symptoms; unless we wish to see, as I have often seen in the hospitals, the patient cured of the tetanus to die by opium. The physiological action of mercury is exerted upon the salivary glands, and with the earliest precursory symptoms of salivation, the blood has already lost some of its morbidly plastic character. It is indeed remarkable to what an extent acute inflammation becomes relieved, upon the appearance of the precursors of salivation, and how long these are in making their appearance in intense and essentially exudative inflammations, as iritis, peritonitis, and especially puerperal peritonitis. In this last we are sometimes surprised at finding the abdomen, which the evening before would not endure the weight of the clothes, supporting next day firm pressure of the hand, the precursory symptoms of salivation having manifested themselves in the interval. These are indeed the signs of the system having become sufficiently saturated with the mineral, which must be left off as soon as they appear, our object not being, save in very rare and obstinate cases, to excite actual salivation. Instead of then pushing on the mercury, if the disease does not yield, we must, in the case of inflammation, have recourse to other antiphlogistics; and in the case of syphilis, to iodine, sudsorifics, &c., carefully limiting the regimen, and avoiding exposure to cold. When, however, the precursory symptoms are dissipated, and the disease has not yet yielded, we may turn again and again to the mercurial treatment. In syphilis this is almost always necessary.

It is from the non observance of the above rules, that so much mischief has been caused by this remedy, and so much prejudice has been raised against it. The excitement of profuse salivation is especially mischievous. The anti-plastic action of the drug may, after long use, so diminish the coagulability of the blood, as to produce a *mercurial scorbutus*, very difficult to cure. *Marasmus* may likewise be produced, especially in children and aged persons, if mercury be employed sufficiently long to induce ptalism or diarrhoea, or the two conjointly. Calomel, particularly, must be given to such subjects with great care. It is not sufficient to withhold it when salivation or purging already exist; but at every visit the condition of the salivary organs and digestive tube must be carefully enquired into. From neglect of this precaution, infants often suffer severely from the prolonged use of calomel.—*Medico-Chirurgical Review*, Jan., 1847, from the *Revue Medicale*, Nov., 1846.

## SURGERY.

*Operation of Myo-Tenotomy.*—[There can be little doubt that in this, as in all other new operations, there is some risk of abuse, from the want of a due consideration of the cases to which it is safely applicable. It must therefore be a matter of interest to the surgeon, to know what are the veritable indications for its performance, and what are the cases in which mechanical means alone will prove sufficient.]

According to Neumann there is but one pathological condition which is indicative of the propriety of the section of tendons; this is *muscular retraction*. When this is not present, whatever be the state of the parts, the operation will not be followed by its

expected results. The muscular retraction referred to manifests itself in general, by a tension and hardness of the tendon or its muscles, which cannot be accounted for by the state of the affected limb. We must be careful not to confound the *retraction* of a muscle, with a *shortening* of its fibres. To shew the importance of a due distinction of these conditions, it will suffice to glance at one of the affections for which tenotomy is most frequently employed, *pes equinus*. A man in perfect health is able, at pleasure, to induce such contraction of the gastrocnemii muscles as shall elevate the heel as much as is commonly seen in *pes equinus*. There is no proof, therefore, that the tendon is *shortened* in this affection; it is only *retracted*, and tenotomy destroys the morbid retraction and rigidity of the fibres, without necessarily causing the elongation of the tendon. And even if it did so, the elongation would be inconvenient, for although the patient would be able to place his heel on the ground, he would walk defectively. Again, it is necessary to separate *morbid retraction* of the muscles and tendons from that which occurs in the aponeurosis and ligaments, forming a *real shortening*.

Among the diseases which require tenotomy, and which depend upon muscular retraction, the author establishes two classes,—those which are general, or those which may appear indiscriminately in any part of the body, and those which are confined to particular localities. The first class includes paralysis and ankylosis, when these are accompanied by permanent retraction of the muscles; certain accidents to which the joints are subject, and which are always accompanied by muscular retraction, such as insensibility, sensation of cold, &c. In the second class he comprehends strabismus, ptosis, tortuosity, club-foot, and articular retractions.

Another important question is whether, when several muscles are simultaneously retracted, they should be operated upon at one or several different times. On this point the author is opposed to M. Guerin, and affirms that it is better to divide several muscles at once if they belong to the same region. If another articulation or limb requires an operation, it should, he observes, be always at an interval of a week at least from the former one.

The author further states, that tenotomy should never be performed on infants under a year old. The operation for strabismus ought not to be recommended before the age of eight or ten years, as up to this time the resources of nature may prove sufficient. So also for club-foot, we ought never to operate before the age of twelve years, because previously to this age a cure may be accomplished by mechanical means alone. After the age of sixty the author thinks tenotomy useless. Inflammation of a retracted articulation is a potent contra-indication to an operation in all cases.—*Casper's Wochenschrift*.

*Treatment of Ganglions, or adventitious Cysts on the Tendons about the Wrist or Foot.*—By F. C. SKRY, Esq., F.R.S.—A puncture with the point of a small lancet is a less painful and more certain remedy than a blow. The puncture may be sufficiently large only to allow the contents to be pressed through. A pad of lint, bound down with adhesive plaster firmly applied, will almost invariably destroy the cavity in twenty-four hours.

A case occurred in the early part of the summer, which may serve to remind us that even these cutaneous cysts will not bear rough treatment. The cyst, which was rather unusually large, occupied the back of the wrist in a youth of 18. I punctured it several times, but it returned. I then passed through it a very fine thread. I was, unfortunately, absent from the hospital on the day of his next visit, and the thread remained for a few days beyond the usual period. Inflammation followed, of a severe kind, and the youth became an inmate of the hospital for some weeks, where, having recovered from the attack of inflammation, he returned to the out-patient room with his original malady.—*Medical Gazette*.

*Straw Splints for Fractures.*—At a recent meeting of the Surgical Society of Ireland, Mr. Tuffnell exhibited a form of splint which he had been in the habit of using for some years, and which he believed had first been invented by Baron Larrey. It is made by filling a linen bag of the size of the splint required, with unbroken wheat straw, that used in the country for thatching being the best. The straw must be cut off at the length to fit the limb, and the open end of the bag sewn up. The splint thus made combines the double advantage of being both splint



and pad in one, and possesses the following advantage over the wooden or whalebone splints commonly in use. When lotions are used to subdue the inflammation in a recent fracture, the apparatus need not be removed from the limb, as evaporation takes place as rapidly through the straw as if the limb was lying unconfined. No padding being used, the hard lumps formed by tow or wool that has been wetted and allowed to dry, and which cause such pain and inconvenience to the patient, are avoided; and if undue pressure is sustained by any projecting process of bone, by inserting the point of the finger, and rolling the straws one upon the other, a hollow is at once formed for the part. In fractures of the upper arm, Mr. Tuffnell said he had, in using the wooden splints and pads, found great inconvenience from the difficulty of keeping the four angles in contact, especially at night, and in muscular arms, the edge of one splint rolling over the other, and at once displacing the whole apparatus; whilst the straw splints, when bound on the limb, so embraced it as to keep up one uniform pressure on all sides, and in fractures of the forearm acted most beneficially by pressing the muscles into the interosseous space more and more as the fillets or bandages were tightened. These, he said, were advantages he had found to result from the common use of the splints; but what he had to adduce most in their favour was the power they gave to the military surgeon and country practitioner of forming at once, in almost every situation in which he could be placed, an efficient contrivance for the treatment of all ordinary fractures; whilst for hospital practice their cheapness was of no inconsiderable advantage: and, in compound fractures, by directing the nurse to take out the soiled straw, wash, and re-fit the case with fresh, at each dressing the surgeon was enabled, at the very least expense, to have a clean apparatus, and thus avoid the necessity of keeping a source of effluvia near the patient, at a period when his recovery and well-being so mainly depend upon pure ventilation and fresh air. In fractures of the forearm, by substituting a fillet and buckle for the ordinary tape or bandage, the patient is enabled to tighten or relax the pressure of the splint according to his own sensations of uneasiness or comfort, and the lightness and regulated pressure make them much less irksome than the old-fashioned wooden splint.

Mr. McCoy approved very highly of the apparatus; and Dr. Jacob said it appeared to him to be a most admirable contrivance. At the first glance, one might be inclined to think it not sufficiently strong; but, on testing the apparatus it was found a most powerful one; and, as Mr. Tuffnell had remarked, the facility of moving the straw, so as to accommodate the splint to the inequalities of the limb, was a very marked advantage. He looked on Mr. Tuffnell's suggestions as most valuable for securing an extemporaneous mode of coaptation, more particularly to gentlemen who are to practice in the country, where there is generally so much difficulty in persons accommodating themselves in urgent cases, sudden as fractures usually are. He had seen an old hand furnish a very efficient splint in the small fractures of children. Then there was the cover of an old book, but even in obtaining this there is often a good deal of difficulty when the inmates of the house a man is called to, are not of literary habits. A capital extemporaneous splint, which Dr. Jacob had seen used in the fractures of children, was the fresh bark of a tree taken off while the sap is rising. It fits admirably, just like a paste-board soaked in water. He had seen a case managed in this way by a common bone-setter, and the whole thing turned out in a way that would do credit to any of our metropolitan surgeons.—*Dublin Med. Press.*

## MIDWIFERY.

*Remarkable Case of Protracted Lactation.*—(Gloucester, Feb. 5, 1847).—Mrs. P., aged 39 years October 28th, 1846, never had a sick day since her marriage Dec. 9th, 1826, except the usual sickness consequent on parturition. During this period she has given birth to eight children, all of whom are now living and in perfect health. The order of their births is as follows:—Sept. 5th, 1827, female; Sept. 5th, 1829, do.; March 28th, 1832, do.; April 1st, 1834, do.; Nov. 11th, 1837, do.; April 3d, 1841, male; April 17th, 1844, do.; Nov. 3d, 1846, female. Mrs. P.'s only brother and sister

lived to adult age, and both died of *tubercular phthisis*. Both parents also died of the same disease. She was married young, and at the time considered a remarkably slender girl, being subject to cough upon the slightest exposure. She has been constantly nursing for a period of nearly twenty years—never weaning one child till the birth of another compelled her to, for the convenience of the infant. More than once, when in labor, I have seen her child of the last birth at the breast.

From a solitary case of this kind, I would not draw a single inference; but should some of your numerous correspondents, from the abundance of their experience, contribute for the Journal similar cases with a like favourable result, might we not infer, contrary to the generally-received opinions of medical men, that *protracted lactation*, especially during pregnancy, possesses a prophylactic power, even when there exists a well-marked hereditary predisposition to pulmonary disease? I. P. SMITH.

*Boston Medical and Surgical Journal.*

*Midwifery Statistics.*—A reviewer in the March number of the Archives Générales gives the following general results of Midwifery Statistical Tables, recently published in the Italian and English Journals. In 47,116 labours, twins occurred 446 times, (9.4-10 per thousand,) triplets four times, (1 in 10,000.) There were 40,233 head presentations, (969 per 1000,) of which 40,016 were vertex, and 187 face. There were 1065 breech or footling presentations, (27 per 1000,) and 154 transverse ones, (4 per 1000.) Of these labours, 46,632 terminated naturally, (989 per 1000,) and 484 (11 per 1000,) artificially,—viz: 221 by means of the forceps, 89 by craniotomy, 54 by turning, and 20 by vaginal or uterine hysterotomy.—*Medico Chirurgical Review.*

*Follicular Disease of Vulva.*—Arg. nit. and nitric acid are of no use. Hydrocyanic acid lotion is serviceable, or an ointment made of two drachms of prussic acid and a scruple of diacetate of lead with two ounces of coconut oil. The parts are to be first washed with infusion of roses, and the ointment applied two or three times a day on lint.

Or try a lotion of lime water with opium; or make a poultice of bread, saturated with decoction of conium leaves, to a pint of which add two drachms of the liq. plumbi diacet.

When irritation is excessive, prescribe vapor-baths, either simple or medicated with sulphur. Attend to general health, order a nutritious but unstimulating diet; avoid wine and porter; give milk with lime water; keep the patient at rest; forbid sexual intercourse. There should be change of air. Give the vegetable tonics, as cascarrilla, cubumba, cinchona, sarsaparilla, &c.; keep the bowels open with small doses of magnes. sulph. in infusion of cascarrilla, chamomile. When the symptoms are decidedly abating, give a mild mercurial course with sarsaparilla. (Mr. Oldham.)—*Braithwaite.*

## MEDICAL JURISPRUDENCE.

*On Wounds from Fire-Arms without Ball.*—By PAUL SWIFT, M. D., Philadelphia.—There is obviously a wide spread popular error in relation to the effect of the explosion of gunpowder without ball; and even professional writers on this subject are not very definite in regard to the distance at which a pistol, or other fire-arm, so loaded, may be discharged without inflicting a dangerous wound.

The popular notion and language is, that the piece is not loaded unless it be charged with ball, slug, or shot, as well as powder, and that its discharge is quite safe even when held near the person. Moreover, when wounds do occur from such discharges, all parties seem quite sure that the *wad* is the immediate cause. The following case will illustrate the prevalence of this opinion and its fallacy.

On last New Year's eve, at the Good Will Engine house, Wm. Simler, a minor, playfully, but heedlessly, fired a pistol

charged with powder only, at his friend and companion Robert W. Pitt, inflicting a serious wound. Pitt, staggering into the arms of his friends, cried out, "I am shot." Simler, thinking him frightened, but not harmed, said, laughing, "It was not loaded," or, as another witness testified, "It had no ball in it." The wound was on the fleshy part of the left hip, above and behind the trochanter major, about one inch in diameter, and four inches in depth; the integuments were destroyed, and the muscles presented a blackened, mangled mass: it bled but little, and was carefully probed with the finger which readily passed to the bottom of the wound. No untoward symptom arose till the sixth day, when tetanus in its most distressing form, opisthotonos, supervened, and he died on the morning of the seventh.

On a careful post-mortem examination, no foreign substance was found but a minute fragment of woollen cloth, about two inches from the surface, and *grains of gunpowder, with which the wound, through its whole extent, was blackened.*

Thus it was evident that this fatal wound was caused by the explosion of gunpowder in a pistol of ordinary size; a wound at least four times as large as a ball from the same instrument would have caused; and so mangled were the tissues, through this great extent, that vitality was utterly destroyed. Had the unfortunate young man lived, it must have been through great suffering, necessarily attending the tedious and exhausting sloughing of the dead mass.

At the legal investigation which followed, there was some discrepancy in the testimony in regard to the distance at which the pistol was held when the wound was inflicted, the witnesses differing from one foot to two or three yards; nor is this very strange, as it occurred in the night, in a place not well lighted, and in the midst of a moving throng of some twenty individuals. The patient himself, however, declared his belief that the weapon "almost touched him." The pistol was said, by Simler, to have had a paper wad; but no wad was at any time found, and the evidence given at the time rendered it quite probable none was used.

Being one of the professional attendants in this case, I was somewhat surprised at the character and extent of the wound; it was obvious no ball could have produced it; nor was it conceivable that a paper wad could have caused such extensive lesions. A considerable research in works on medical jurisprudence failed to give satisfaction, and though the circumstances forced upon my mind the inference that a heavy charge of powder, exploded near the part, had alone caused the mischief, yet, as young Simler had been bound over for trial before a criminal court, it seemed very desirable to possess farther data in the premises. With this view I sought, and, through the courtesy of the officers of Jefferson Medical College, obtained the facilities for making the following experiments, which were used at the subsequent trial, and are now offered to the public with a hope that they may be useful in future medico-legal investigations of this nature. The pistol—the same used by Simler—was wadded with paper, and has a bore about four inches in depth and six lines caliber.

*Experiment 1st.*—Pistol with an ordinary charge was held 12 inches from fleshy part of hip, the part being covered with one thickness of broadcloth and a twilled cotton cloth under it—Clothes lacerated, and skin abraded; wad on the floor, on fire.

*Experiment 2d.*—Distance 6 inches; parts covered as before, clothes lacerated; wad lodged one half inch below the surface.

*Experiment 3d.*—Distance 2 inches—wound ragged, blackened with powder, and penetrating to the bone—one and a half to two inches—wad was found immediately beneath the integuments, and somewhat on *one side of the principal wound*—parts covered as before.

*Experiment 4th.*—Distance one and a half inch from the

ribs of the right side—no covering of cloth—penetrated the cavity of the chest, the wad passing through the intercostals between the ribs.

*Experiment 5th.*—Distance the same—no covering of cloth, the integuments removed—wad penetrated the thorax, *carrying away a transverse portion of the rib.*

The subject, about 35 years of age, a male, not recent, had undergone a preserving process with chloride of mercury, considerably hardening the muscles; it was also much emaciated.—*Medical Examiner.*

## MATERIA MEDICA AND PHARMACY.

*Syrup of Iodide and Chloride of Iron.*—By S. BATTLE, Esq., of London.—As iodine is at present so extensively employed, and maintains so high a character as a remedial agent, it appears rather singular that some of its most efficient combinations are still so little used. Amongst these, the iodide of iron is one which has not yet attained that rank in professional estimation that its merits claim. This seems in part owing to the process by which the London Pharmacopœia directs it to be prepared, its great tendency to decomposition, and the difficulty of its preservation. The iron possesses a greater affinity for the oxygen of the atmosphere than for iodine, in consequence of which the latter is set free, and when the preparation is exhibited in such a state, the stomach is frequently incapable of bearing what might otherwise prove a valuable tonic. This tendency to oxidation in the iron, and liberation of free iodine, is indeed prevented by using the syrup, the sugar in which has the property of preventing protoxides, protiodides, and protochlorides, from absorbing oxygen, and passing into peroxide, in which states iron exerts but little action on the system.

Practical men, however, have remarked, that the iodide of iron, even when given in the best form, has sometimes failed to produce that speedy and decisive chalybeate influence on the system that other salts do, though in other respects an agreeable and elegant form. This observation is readily explained on reference to the constitution of the salt, in which the amount of iron is less than one-fourth of that of the iodine, the combining proportion of the former being twenty-eight, while that of the latter is 126. As iodine is incapable of entering into combination with a greater proportion of iron, in order to increase the quantity we may substitute another salt of iron, isomorphous in its crystal, and analogous in constitution in the protochloride. These two salts may be mixed in any proportion without decomposition, and thus present an elegant and powerful preparation of iron, while the iodine, by its action on the glandular system and secretory apparatus, tends to prevent that cerebral plethora which the salts of iron, when given *per se*, so frequently induce.

The syrup of the iodide and chloride of iron, which it is the object of the preceding remarks to introduce to the notice of the profession, is prepared by first forming a solution of iodide of iron. This is effected by diffusing iodine in three or four times its weight of cold distilled water, and agitating for ten minutes with half the quantity of iron filings, added cautiously and gradually, when the colour changes from dark purple to a deep green, as combination takes place. The protochloride of iron is next formed, by acting upon iron filing with hydrochloric acid, specific gravity, 1.160. A copious disengagement of hydrogen gas ensues, and continues for several hours, during which the chlorine combines with one equivalent of iron, the fluid becomes neutral to test paper, and we obtain a solution of a green colour, also with a shade of blue. The two solutions are now mixed together, and so much refined sugar as will form a syrup.

The syrup ought to be of a pale green, representing the proto-salts of iron. The proportions of the salts in solution are so adjusted, that each fluid drachm of the syrup contains three grains of combined iodine and nearly four grains of iron, united partly with the iodine and partly with the chlorine. The following formula may afford an eligible mode of exhibition:—Syrup of iodide and chloride of iron, two drachms; syrup of orange-peel, four drachms; infusion of cascarrilla, four ounces. Mix for four draughts, one to be taken twice daily.

The syrup of iodide of iron, and syrup of chloride of iron may be made and kept separately, when they can be mixed in any



proportion, according to the amount of iron or iodine the practitioner may wish to administer.—*Lancet*.

**Arseniate of Quina.**—This salt, first prepared by M. Bourières, has latterly been much used in France in the treatment of obstinate intermittents, and, it is stated, with much success; the chief obstacle to its more general employment being, according to Dr. Boudin, its extreme bitterness. It is readily prepared as follows: dissolve half an ounce of sulphate of quina in boiling water, and precipitate with ammonia; wash and dry the precipitate, and dissolve it with the aid of heat in three ounces of distilled water, containing two scruples of arsenious acid in solution; as the solution cools, crystals of arseniate of quina are deposited, which are to be dissolved in distilled water and recrystallized. It is a light, white salt, crystallized in brilliant satiny needles. It is soluble in water, but more so in boiling than in cold water; it is also soluble in weak alcohol, but is insoluble in absolute alcohol or in ether. The dose of it is from one to two grains in divided doses in the course of twenty-four hours. It is usually given in solution in distilled water, to which a little simple syrup may be added.—*Philadelphia Medical Examiner*, Oct., 1846.

**Method of Finding Specific Gravities.**—The ordinary mode of taking the specific gravity of fluids by means of the thousand-grain bottle has been found inconvenient at times, and the use of it for acids very awkward. Whenever I wish to ascertain the specific gravity of any small quantity, say 100 grains, I make use of an upright, accurately graduated minim measure, take its counterpoise, and fill it exactly to 95 minims, which is the measure or bulk of 100 grains of distilled water as unity, not the density of the fluid to be examined, and compare accordingly. Suppose the weight of sulphuric acid is to be taken, all one has to do is simply to measure out 95 minims, place the counterpoise of the measure in one pan, and fluid in the opposite. The difference of weight compared with unity gives the specific gravity with very little trouble, and tolerable accuracy.—*Mr Horsley, in the Chemist*.

THE

**British American Journal.**

MONTREAL, MAY 1, 1847.

**United States National Medical Convention.**—The adjourned meeting of the National Medical Convention will be held in the city of Philadelphia on the 5th inst. It will be recollected that a general meeting of the Profession of the United States took place in the city of New York, on the 5th of May, 1846. A resumé of their proceedings will be found in the June number of this Journal for that year. At that meeting committees were appointed to draw up reports on certain resolutions, which we published at the time. These resolutions embodied the following subjects:—1. The organisation of a National Institute. 2. The adoption of a uniform and elevated standard of medical education. 3. The standard of acquirements to be exacted, before entering upon the study of medicine. 4. A code of medical ethics. 5. A regular system of enregistering births, marriages, and deaths. 6. The separation of the business of teaching from licensing in the same hands. The different medical institutions in the United States have already nominated their delegates, and, as far as we can judge, unanimity prevails throughout the Profession on the subject.

**King's College, Toronto.**—At a Convocation held in the University Hall, to grant prizes and certificates of honours to fortunate candidates in the faculty of medicine; prizes and certificates were awarded to the following gentlemen.

SENIOR CLASS.

*Anatomy and Physiology*, Askin (C. I. S.), prizeman. *Medicine*, Salmon (S's), prizeman. *Surgery*, Harvey (Jno.), prizeman.

JUNIOR CLASS.

1st Class, *Practical Anatomy*, Herod (C. S.) prizeman. 2nd, Askin, (C. I. S.), *Anatomy and Physiology*, Checkly (R.) prizeman; 2, Cronyn (T.); 3, Chewitt (W. C.) *Chemistry*, Marling (F.), prizeman. *Medicine*, 2, Herod (C. S.) *Obstetrics*, Salmon (J's.), prizeman. *Materia Medica*, Cronyn (T.), prizeman.

2nd Class, *Practical Anatomy*, 1, Nation (Jno.) 2, Chewitt (W. C.) *Anatomy and Physiology*, Nation (Jno.) *Medicine*, 1, Chewitt (W. C.); 2, Harvey (Jno.) *Surgery*, Hagerman (J.), B. A., and Herod (C. S.), æquales. *Practical Chemistry*, Checkly (R.)

The distribution of prizes was followed by appropriate addresses from the Vice-President, and from the Honourable and Right Reverend the President of the University.

**Harvard University.**—Dr. Warren, who has been for nearly forty years the Hersey Professor of Anatomy and Surgery in Harvard University, has lately resigned his Professorship. A meeting of the President and Fellows of the University was held in Boston, on the 27th Feb., at which his letter of resignation was accepted, and in consideration of his faithful and valuable services, he was unanimously elected Emeritus Professor of Anatomy and Surgery. The corporation of the University have since appointed three new Professors; two of whom are to be attached to the Massachusetts Medical College in Boston, and one to the University. The new incumbents are, Oliver W. Holmes, M. D., Professor of Anatomy and Physiology; and John B. S. Jackson, M. D., Professor of Pathological Anatomy; Jeffries Wyman, M. D., having been appointed Hersey Professor of Anatomy at Cambridge in the room of Dr. Warren. The new incumbents are all highly spoken of, in the Boston Medical and Surgical Journal, as having already attained distinction for their anatomical labours.

**The Illustrated Flora.**—We have received No. 1, vol. i., of this new periodical, edited by Dr. Newman, the scientific editor of the Illustrated Botany, which has completed its first volume, but the editorship of which has now passed into other hands. Dr. Newman now confines his labours to the "Flora," and the specimen of the work before us proclaims his unwearied energy, and his taste. The work is divided into four departments: Floral, Medical, Introductory, and Biographical. Under each head, those necessary details are given which will render it a valuable acquisition to the library of the Horticulturist, the Botanist, the general reader, and the Physician. Each number contains 50 pages of letter press, of royal octavo size, and is furthermore enriched by six plates, from the lithographic press of Lewis and Brown of New York, richly coloured after nature by the hand; the Price is \$3 per annum, and the wonder to us is, that a work of such merit, and so highly embellished can be furnished to subscribers at so low a price. It deserves perusal and a generous support.

*British and Foreign Medical Review, January, 1847. Braithwaite's Retrospect of Practical Medicine and Surgery, Part 14th, 1847. London Lancet, March, 1847.*—We have received the American reprints of the foregoing valuable periodicals. To the characters which they severally enjoy, we can add nothing. They are furnished to subscribers at the rates of \$4, \$2, and \$6, per annum, respectively. Subscriptions are received by Mr. Lay, Agent, at R., and A. Miller's, St. Francois Xavier Street, in this city; from whom also the Illustrated Flora may be obtained, the subject of notice in the preceding article.

*The Annalist, a Record of Practical Medicine in the City of New York, edited by W. C. Roberts, M.D.*—Having lately received thirteen numbers of this Journal, we are enabled to speak confidently of its character. We consider it a useful and really valuable addition to the medical periodical literature of the States. The selections are made with care, and the editorials evince sound judgment, vigorous expression, and a high sense of professional honour. We cannot, under the present editor's able management, doubt its success, and the attainment of an enviable position among its contemporaries.

*Meteorological Register for Montreal.*—We have to express to Dr. Gibb, lately of the Montreal General Hospital, our acknowledgments for the care and assiduity with which he has for the last two years taken the daily observations of the winds, which have regularly appeared in the meteorological table for this city, published in this journal. Nor less are we under an obligation to W. Skakel, Esq., for the care with which the other departments of the same table have been filled up. On Dr. Gibb's return from Great Britain, whither advancement in his profession is now calling him, we hope he will re-commence his favours in support of our undertaking.

*Licentiates of the Medical Boards.*—In consequence of the difficulties which we have experienced in obtaining returns of those who have passed their examination at the Medical Boards, we shall in future depart from our usual custom, and give the official announcements of the dates of license from the Canada Gazette. The last number of this Journal contains the following:

“Montreal, Feb. 17, 1847.

“His Excellency the Governor-General has been pleased to grant a Licence to Dr. J. Salmon, of Simcoe, to practice Physic, Surgery, and Midwifery, in the Province of Canada.”

Several editorial articles are omitted, in consequence of the large space occupied in this number by original communications.

#### CORRESPONDENCE.

To the Editor of the British American Journal.

SIR,—It has been whispered abroad that the “School of Medicine” are about to confer their diplomas on some youths who are probably made to believe the

piece of parchment they receive is of *real value*. If you have reason to think the report true, had you not better, in the next number of your valuable journal, expose the fallacy of any such belief as that their diplomas are of any intrinsic value at all.

April 15, 1847.

AN EDINBURGH SURGEON.

[In publishing the above, we have to observe, that rumours of similar proceedings have reached us also. After what we have said on the subject, we could hardly have supposed that the School of Medicine would have continued the practice, or that parties could even be found willing to part with their cash for a spurious document. If, as it appears, we cannot stop the practice, we will be enabled at least to let the public and the profession know who those are who hold the valuable documents.—Ed.]

#### BOOKS, &c., RECEIVED DURING THE MONTH.

- Dublin Medical Press. March 10, 17, 24, 31.  
 The Annalist, a record of practical Medicine in the city of New-York. Nos. 1 to 13.  
 New-York Medical and Surgical Reporter. March 27, April 3, 10.  
 American Journal of Insanity. April.  
 Boston Medical and Surgical Journal. March 31, April 7, 14, 21.  
 Stockton's Dental Intelligencer. March.  
 The Illustrated Flora, edited by J. B. Newman, M.D., New-York. No. 1.  
 Medical News and Library. April.  
 La Lancette Canadienne. April 1, 15.  
 Republication of London Lancet. March.  
 British and Foreign Medical Review. January.  
 Braithwaite's Retrospect. Part 14.  
 The American Journal of the Medical Sciences. April.  
 An Address to the class of Graduates of the College of Physicians and Surgeons of the University of the State of New-York, delivered at the commencement, March 11, 1847, by Alexander H. Stevens, M.D., President and Emeritus Professor of Surgery. New-York, 1847.  
 Missouri Medical and Surgical Reporter. March.  
 Buffalo Medical and Surgical Journal. April.  
 Valedictory Address to the Graduates of the Jefferson Medical College, by Robley Denglison, M.D. Philadelphia, 1847.  
 London Medical Gazette, March 19, 26.  
 Provincial Medical and Surgical Journal, March 10.

#### TO CORRESPONDENTS.

*Dr. Elliot's paper on “the effects of constitutional and local irritation upon the formation and eruption of the Teeth,” read at the last meeting of the Medico-Chirurgical Society of this city, has been received, and will appear in the next number. A paper from Dr. Marsden, with the Meteorological Observations at Nicolet for a series of years; and another from Dr. Evans of Richmond, (case of Frambesia,) have also come to hand. We beg to remind Dr. Grasset (Toronto) of his promise contained in his letter, dated 20th instant.*

Errata, in the Meteorological Register of Toronto.—Our readers are requested to preface the degrees, opposite the following months in the respective years, in the small bottom table of annual values, with the negative signs:—1810, December, January, February; 1811, January, February; 1843, February; 1844, January; 1845, December, January, February; 1846, January, February; 1847, January, February. For the “lowest temperature” in January, on the 21st, read  $-2.9^{\circ}$ , instead of  $2.9^{\circ}$ ; for the “lowest temperature” in February, on the 24th, read  $-0.9^{\circ}$ , instead of  $0.9^{\circ}$ ; and in the same month, before the temperature for 7 a.m., on the 24th, in the body of the table, read  $-0.3^{\circ}$  for  $0.3^{\circ}$ .

**BILL OF MORTALITY for the CITY of MONTREAL, for the month ending MARCH 31, 1847.**

DISEASES	Male.	Female.	Total.	Under 1.	1 & under 3	3 — 5	5 — 10	10 — 15	15 — 25	25 — 35	35 — 45	45 — 55	55 — 75	75 upwards
EPIDEMIC OR INFECTIOUS,.....	Small Pox,.....	2	2	2	1	1								
	Measles,.....	1	1	2	1	1								
	Fever,.....	9	6	15	3	2	1	4		1	1			
DISEASES OF BRAIN AND NERVOUS SYSTEM,.....	Convulsions,.....	3	2	5	3	1								
	Dentition,.....	4	5	9	3	6								
	Paralysis,.....	1	1	2	2									1
	Water in Brain,.....	2	2	4	2	2								
DISEASES OF RESPIRATORY ORGANS,...	Consumption,.....	19	24	43	11	3		3	6	6	9	1	3	1
	Croup,.....	3	1	4	3	1								
	Hooping Cough,.....	1	1	2	2									
	Dropsy,.....	5	3	8	1	1				1		1	1	3
DISEASES OF ABDOMINAL VISCERA,	Liver Complaint(?),.....	2	2	2				1		1				
	Diarrhœa,.....	2	2	2										
	Worms,.....	1	1	1	1									
	Inflammation,.....	6	11	17	4	2	2	2		4	2		1	1
	Sudden Death,.....	1	1	2							1			3
OTHER CAUSES AND DISEASES, AND DISEASES NOT SPECIALLY DESIGNATED,.....	Debility,.....	1	7	8										5*
	Unknown,.....	4	2	6	1				1	1	2	1		
	Enlargement of the Heart,.....	1	1	1								1		
	Childbirth,.....	1	1	1							1			
	Still-born,.....	2	1	3	3									
	Ulcer,.....	1	1	1	1				1					
Accidental,.....	1	1	1	1										
<b>Total, .....</b>	<b>69</b>	<b>71</b>	<b>140</b>	<b>37</b>	<b>23</b>	<b>4</b>	<b>6</b>	<b>5</b>	<b>9</b>	<b>14</b>	<b>18</b>	<b>3</b>	<b>14</b>	<b>7</b>

\* One a female Canadian, æt. 103.

**MONTHLY METEOROLOGICAL REGISTER AT MONTREAL FOR MARCH, 1847.**

DATE.	THERMOMETER.				BAROMETER.				WINDS.			WEATHER.		
	7 A.M.	3 P.M.	10 P.M.	Mean.	7 A.M.	3 P.M.	10 P.M.	Mean	7 A.M.	Noon.	6 P.M.	7 A.M.	3 P.M.	10 P.M.
1,	+12	+22	+16	+17-	29.40	29.56	29.73	29.56	W.	N. W.	W.	Fair	Fair	Fair
2,	" 9	" 22	" 12	" 15.5	29.88	29.89	30.10	29.96	W.	N WbyW	N WbyW.	Fair	Fair	Fair
3,	" 6	" 25	" 20	" 15.5	30.17	30.11	30.07	30.12	W.	W.	W.	Fair	Fair	Fair
4,	" 15	" 32	" 22	" 23.5	30.10	30.16	30.23	30.16	N. W.	N. W.	N. W.	Fair	Fair	Fair
5,	" 15	" 31	" 20	" 23-	30.37	30.38	30.44	30.40	N. W.	N. W.	N. W.	Cloudy	Fair	Stormy
6,	" 13	" 34	" 13	" 23.5	30.54	30.52	30.50	30.52	N. W.	N. W.	N. W.	Fair	Fair	Fair
7,	" 12	" 19	" 26	" 15.5	30.22	29.97	29.77	29.99	NW by W	N Wby W	N Wby W.	Fair	Fair	Rain
8,	" 32	" 34	" 19	" 33-	29.86	29.99	30.24	30.03	W.	W.	W.	Fair	Fair	Fair
9,	" 5	" 22	" 8	" 13.5	30.46	30.36	30.25	30.36	N. W.	N.W.	N. W.	Fair	Fair	Fair
10,	" 11	" 27	" 21	" 19-	29.98	29.94	29.89	29.94	N. E	W.	S. WbyW	Cloudy	Snow	Fair.
11,	" 9	" 21	" 12	" 15-	30.10	30.07	29.95	30.04	N. W.	W. N. W.	W. N. W.	Fair	Fair	Fair
12,	" 3	" 19	" 21	" 11-	29.92	29.88	29.92	29.91	N. N. W.	W.	W.	Fair	Fair	Fair
13,	" 8	" 27	" 17	" 17.5	29.98	29.89	29.95	29.94	W.	W.	W.	Fair	Fair	Fair
14,	" 15	" 31	" 19	" 23-	30.00	29.94	30.00	29.98	W.	W.	W.	Fair	Fair	Fair
15,	" 12	" 27	" 13	" 19.5	29.97	29.90	29.99	29.95	W.	W.	W.	Fair	Snow	Fair
16,	" 11	" 25	" 16	" 18-	30.30	30.00	30.02	30.01	W.	W. by N.	W. by N.	Fair	Fair	Fair
17,*									W. by W.	W.	W.			
18,*									W.	W.	W. by S.			
19,*									W.	W.	W.			
20,	" 26	" 34	" 36	" 30-	29.87	29.61	29.27	29.58	W.	W.	S.	Fair	Cloudy	Rain
21,	" 27	" 32	" 20	" 29.5	29.64	29.81	29.90	29.78	N.	N.	N. by E.	Fair	Fair	Fair
22,	" 20	" 23	" 25	" 21.5	29.79	29.67	29.57	29.68	N. E.	N. E.	N. E.	Snow	Stormy	Rain
23,	" 32	" 41	" 35	" 36.5	29.55	29.35	29.30	29.40	N. E	S. E.	S. E.	Cloudy	Fair	Rain
24,	" 34	" 45	" 35	" 39.5	29.30	29.41	29.51	29.41	W. by W.	W.	W.	Fair	Cloudy	Fair
25,	" 34	" 50	" 37	" 42-	29.45	29.32	29.50	29.43	S.	W.	N. by S.	Fair	Fair	Rain
26,	" 39	" 38	" 31	" 38.5	29.50	29.55	29.42	29.49	W. N. W.	N. W.	N. E.	Fair	Snow	Rain
27,	" 29	" 30	" 21	" 29.5	28.78	28.74	29.05	28.86	N. E.	N.	N. W.	Snow	Sn-stor	Stormy
28,	" 12	" 30	" 19	" 21-	29.22	29.50	29.58	29.43	N. W.	N. W.	N. W.	Fair	Fair	Fair
29,	" 17	" 27	" 17	" 22-	29.51	29.40	29.45	29.45	N. W.	N. E.	W.	Fair	Snow	Fair
30,	" 16	" 30	" 19	" 23-	29.49	29.61	29.58	29.56	W.	W.	W.	Fair	Fair	Fair
31,	" 12	" 35	" 16	" 23.5	29.58	29.66	29.79	29.68	N. E.	N. E.	N. W.	Fair	Fair	Fair

THERM. } Max. Temp., +50° on the 25th.  
 } Min. " +3° " 12th.  
 Mean of the Month, 23° 55.

BAROMETER, } Maximum, 30.54 Inches on the 6th.  
 } Minimum, 28.74 " " 27th.  
 Mean of Month, 29.808 Inches.

\* No observations taken on the 17th, 18th, and 19th.

