Hiram Watson Campbell
Faction of old women

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Thesis
Critical Observations
On
The Science and Practice of
Medicine
By
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In whatever circumstances a man may find himself placed, or whatever be the calling or profession which he pursues, the only requires to look with about him to find in his own sphere, something worthy of his approval, and something also which he may feel disposed to condemn. The whole duty of no man can be performed by mere acts of spontaneous acts: whatever any one does, to be well done must previously be submitted to reason and sanctioned by his sense of what is right. Unprejudiced circumstances can estimate the error of a cause which has proved disastrous if the approval of such tribunal has not been sought and received.

This is true indeed that a reference made to it has not unfrequently been itself a cause of error, as when reason or judgment has not been sufficiently educated to perceive the true side of an intricate or delicate question, or when prejudice has misled both reason. Conceived to preponderate towards one side, with influenced perhaps miserable to them individual and irrespective of the tendency of argument. Hence it is the duty of all, especially of professional men, as being educated to direct themselves
themselves of an element calculated to render
their conduct or their teaching in many degrad partial.
Yet so many and so different are the conditions
forms influencing the minds of men that regarding
them as different standing points, we need hardly
be surprised an object should seem to each different
in some respects from the view taken of it by others.
Were this truth remembered there would be
more mutual difference of opinion, and no greater
departure from the truth than there is at present.
In scientific investigations, whether their nature
be speculative or practical, this consideration
ought never to be lost sight of. Indeed it is one of
the great truths disclosed by the history of the past;
for not only have contemporaries differed in their
conclusions respecting many things, but men of
the present entertain different opinions on many
subjects from those who preceded them—this may
be entitled to do by the wealth of appliances
and means which time and the onward march
of science and civilization have enriched them.
However, though not to be forgotten that these pri-
velages increase as time passes, and that what now
seems perfectly uncontroversial truth regarding

some subjects may to those in the future seem
less decided by its only becoming that what
seems true and well supported be not expressed
dramatically, but with sufficient deference and
respect to his contemporaries and successors, having
his opinion, supported with all possible argument
to their condemn to decision.

I have been led to these observations by attention
of the subject which I have chosen for my thesis,
not as an apology for it, but because they have
directly on some points which have been brought
under my notice in lectures at Cambridge and at
the bedside, and which have occurred to me in
my private studies, and which may be of scientific
or practical interest.

"Let those think other who themselves excel," no one
will presume to object, yet so long as thought
and adherence are unfitted, the taught may be
found to "assume force," and sometimes not with
definite reason. There are some men, and examples
are not wanting—among our professors, who have
a great facility of disposing of vexed questions
in a summary way, or who, distinct without
being oracular dictum, and this too without
the
the slightest thought of sacrilege the faith of past generations. Yet, however extensive the knowledge, or acute the discernment of any one, such it must be conceded that no one professing an interest in his studies is entitled to accept conclusions so made, without duly weighing the arguments for and against them, and also, that deference to others who have done much for the advancement of medical science demands that the opinions shall receive such attention as they really merit. The first point to which I shall allude here is the influence of mental impressions on the fetus in utero. This is of no great practical value but of considerable scientific interest. Few matters, if any, doubt that the mental impressions of the pregnant woman are capable of influencing the appearance or constitution of the child, or of producing marks on, and men who have done much in advancing medical science to its present state seldom disbelieved it. The great question with them (and perhaps with us) was - What is the medium whereby such mental impressions are conveyed to the child? This may indeed seem a hazardous
way of proceeding, and a thorough logician might be disposed to step back and ask first, Do any mental impressions really influence the child? and having got this answer in the affirmative, he would then pass to the question formally proposed. But it is not surprising that the starting point of argument was as stated, seeing that the matter was never questioned, and in fact commands an inexcusable faith founded on remarkable coincidences as I shall at present call them—dozens of which could be narrated without intermission by any old woman. Besides, similar coincidences were known to have occurred among the lower animals. Human females explained these by referring them to impressions made on the mind during pregnancy, and further than this, the story of Jacob Laban as told in Scripture was regarded as a warrant for belief in the reality of such influence, though the chapter following the one containing this story sufficiently explains the apparent mystery. Upon the whole then there was perhaps no great error committed by taking for granted that the
mind of the mother really could affect the body of the child. That arrest of mental development and
harmful was not unfrequently referred to the idea.
I have been treating of is no doubt true, and that
coincidence might explain much, I do not
deny; yet I think that neither of these can account
for all remarkable cases which have occurred
which have been accounted for by the popular
and more mysterious manner. Professor Simpson
seems to ridicule the popular faith and the way
in which even eminent men had sought to explain
natural causes of this kind, and in opposition to
Ben. Spr. Enfield Home states that what was
then taken for nerves in the umbilical cord, and
by which they supposed were the media for con-
veying these influences of the mental impressions
from the mother to the child, were not really nerves.
Professor Simpson argues further that because
no anatomist has discovered any nervous
matter in the cord, because in fact the cord has
been proven to be of very low organization - nothing
in it could possibly serve as a medium of
such a nature as would be required to convey
the
the influence of mental impressions. Now at the outset we have a petitio principii, inasmuch as it is presumed that it is impossible that any structure—not nervous—could be a medium. Again, though the end be of slow organization, and merely because anatomists have failed to discover nerve matter in it as yet, I am not sure that we are warranted in concluding that elements of a nervous kind don't exist or will never be demonstrable. And Professor Compton to be consistent ought to reason in the same way on other subjects. If this done many things would be denied because not exactly in accordance with the doctrine because a thing is unseen therefore it is absent. The actual passage of fluids through membrane, or the change of characters of two fluids separated by membrane is demonstrable only by the change of position or of characters, but not during actual transition; and it would be absurd if anyone were to conclude that, because in careful examination the fluid can not be seen in process, therefore there was neither a change of place nor character. In the same way,
very one might argue regarding the presence or absence of nerve in one form or another in the eye. But is it impossible for any other structure to serve as a propagating medium? Let me take an analogous subject, and have a little fanciful reasoning before answering that question. How often, and how striking do we find family resemblance! Too often and too striking to be accounted for by mere coincidence. Burns remarks of his “bonnie bayon dear-bought bees” that “she looks her daddie in the face.” Instances of this family resemblance are found in almost every family, and are far more common than the father’s marks. Yet none can limits of trying to prove the possibility of the child resembling the parent because the resemblance is more than possible it is actual. But it would be found perhaps a difficult matter to account for such a resemblance, and impossible to trace it to the influence of a nervous structure existing between parent and child. That there must be some agency for this resemblance is proven by
by the fact of the resemblance, and now there is sufficient evidence proving that that agency is independent of nerves. Now if in the latter case the influence is promotive or conductive independently of nerves, why should Ref. Swinnom deem them necessary for propagation of the influence of mental impressions? That a most important medium exists between mother and child is certainly sufficiently manifest, and yet be the primary object of such communication to convey the nutritive material to the child, may it not secondarily propagate the influence of mental impressions? I have no theory of my own to propose here, but I would merely suggest that because a subject is not fully understood, surely we ought not merely in that account to deny certain of its conditions or even become sceptical of its existence. It seems to me that the term vital might be of service here. It is at present indispensable to the physiologist as it has some matter through his ignorance with evil of mystery, and if mental impressions were to be closed
clapSED with what have been called vital phenomena, they might be still left for elucidation, to future application and research.

This is another subject of scientific interest in ab...stereics about which there has been much discussion — viz. the formation of what has been called the decidua reflexa. Theory after theory having been alternately proposed and rejected, according as the plausibility of one seemed greater than of another. Professor Simpson has taken it up, and after going over the pros and cons of each he arrives at the conclusion that this membrane can only be formed by the ovulum pushing before it, or entering the uterine cavity, a fold of the decidua vera or hypertrophied lining membrane of the uterus. In proposing this explanation it is presumed that the uterine cavity is rendered patent by the approximation and ultimate coalescence of the parts of the hypertrophied membrane around each fallopian orifice (and os uteri also). I must own that I am indebted to my fellow student Mr. John Smith for the suggestion of a difficulty, the due appreciation of which would seem to offer an almost insurmountable objection to this theory, and I have been surprised.
surprised that Professor Simpson himself has not seen it. It is admitted that the general site of the placentæ is opposite one or other fallopian tube, but that it may occupy almost any part of the inner surface of the uterus as the fundus, anterior, posterior wall, or even the cervix and cervix immediately adjacent. How to explain this theory the fact that generally the position of the placentæ is opposite the fallopian tube, we must suppose that the ovum on reaching the fallopian-atrial orifice does not as one usual supposition would assume, descend between the decidua vera and the inner muscular wall to a considerable extent. This must presuppose that a separation between these organised structures takes place to admit of such descent and further that in cases in which the placenta is attached to the fundus, an actual ascending separation is effected, and that whatever is placenta praevia the descending separation must have previously gone on throughout the whole length of the uterine cavity, in which case the face of the endometrium may have been raised in order to prevent the entire extrusion of the ovum into the
the vaginal canal. Nor not only does the patient condition of the cavity require proof— the nature of the agency by which such separation is effected must likewise be explained. To me Professor Goodaur's theory seems more tenable, as it creates no mucus, without such difficulties, and as the membrane formed according to that theory is practically well suited for all the physiological purposes required of it. Professor Goodaur's theory is that after impregnation the mucous membrane becomes thicker: osteoid and effuses cellular matter, part of which forms alveus on the mucous membrane— another part passing into the uterine cavity and constituting the hydroperitoneum, the former part is of a kind capable of further development from the contained nuclei of the cells; that the ovum passes through an as yet nucleated fallopian-uterine orifice and then gets entangled on the mucous membrane and embedded in the cellular effusion on the mucous membrane; that it remains fixed there by the villous uterine part of the ovum (chorion) being entangled by the effusion, and that at this spot a plastic effusion is thrown up around it so as to envelope it by what afterwards becomes an adventitious membrane called the decidua reflexa. Such membranes are
are frequently met with on mucous surfaces as pathological products, and if they are capable of being formed under pathological conditions why may one not be formed in the way just noticed if the physiological conditions of the uterus under it necessary after impregnation.

The cavity of the uterus must be open until at least the ovum has entered it according to this theory, and this is very probable as different observers have found one, two, or all orifices in occluded, according to the stage at which the different observations were made. Nor would this circumstance of the open condition of the cavity lead to suspect a greater proportion of cases of placenta previa than are really met with, for the ovum being exceedingly small can be readily secured by the cellular effusion and the changes which immediately follow — besides, the hydrophorince offers an obstruction sufficient to hinder the occurrence of placenta previa quite exceptionally. Indeed this theory suggests a good reason why the placenta is found occupying a position opposite one or other fallopian tube, for in passing through the orifice into the uterine cavity it only requires to
slide down the meno membrane for a short way, or to drop entirely free to the opposite side into the second for itself either position.

Professor Simpson argues that traces of mucous follicles can be seen on the decidua reflexa and that therefore it is of higher organization than a mere adventitious membrane. These traces are said to be most distinct on the part of the membrane near the place whence the alleged reflection has occurred, the traces on the other parts of the membrane being nearly or altogether obliterated by the distension of the reflected part from increase of its contents. But I think the distension must be equal at all points, and cannot conduct and why obliteration should take place at one point without extending all over the membrane. The apparently higher organization of this than of other structures of the same class may perhaps be explained as follows:

The parts of the projected & elastic lac will first come into contact with the true lining membrane of the uterus immediately around the part whence the projection is made, and as the approximation becomes closer & closer we might expect to find after the
* R. J. Smith says: 

Thus, just formed, the decidua reflexa appears to differ from the decidua vera in being composed chiefly of cell, while the decidua vera is characterized by the tubular glands of the uterus.
the so-called traces of organization, as the result of this application of the plastic effusion to the free surface of a membrane on which are follicular orifices exist—the plasma having become membranous while the follicular impressions exist upon it.

A subject of perhaps greater interest than either of those to which I have referred in preceding pages is the alleged change of type of inflammations, and the consequent change in the treatment of Pneumonia, one of the inflammatory diseases. Professor Bennett is disposed to attribute a greater success attending the present mode of treating this disease as compared with the former (i.e., by the change is said to have occurred), exclusively to the improved means of diagnosis, and the advancement made in the pathology of the disease. He discovers any change in its character as common to the changes said to have taken place within the last thirty-five years in the character of inflammations. Finally, in seeking to establish the proposition that inflammation is the same now as it has ever been, and that the analogy ought to be established between it and the
"the varying types of fever is fallacious," he throws entirely out of view the fact that there is a constitutional as well as a local element in inflammation. In reference to this matter, Professor Christian says—

"The late confined himself in a great measure to the local portion of the disease which he investigated overlooking the fact that it has a constitutional ingredient also. Fever and acute inflammation alike consist of local derangement and constitutional disturbance. The local affection in inflammation and the constitutional one in fever being primary. The essence of the local inflammation in pneumonia, no doubt, as Dr. Bennett forcibly urged may be quite the same now as it was when Dr. Gregory practiced and even when Hippocrates flourished. But it does not follow that the constitutional accompaniment, consisting of a disturbed circulation and a disturbed nervous system, has likewise been always the same. Nor did Dr. Bennett advance a single argument to prove it to be so. Though in this respect lies the whole essence of the matter. For in the treatment of pneumonia, for example, the constitutional element of the disease cannot be discarded merely because
"because it happens to be secondary to the local 'morbid action. On the contrary it is often the main object of regard in the treatment, because on the one hand it may be such as to aggravate local malaction or, on the other, to forbid remedies otherwise available for its removal."

To these remarks Professor Bennett replies: "D. Christian, like Dr. Warden, points out that I have advanced no argument to show that the febrile phenomenon must always be the same. But I humbly think it is not for me to show that the human constitution is incapable of undergoing alterations. The onus probandi must be laid on those who assert that any such change is sufficient to account for the remarkable modifications which have taken place in medical practice during the last twenty years. Now I think that the onus probandi has been borne and offered by Professor Christian in the paper from which I have quoted and to which Professor Bennett himself alludes. In it is evidenced there has been given that other acute diseases have undergone a marked change, as well as inflammation, and that such change was so decided as to demand a modification of their treatment. D. Christian says in the same paper, 'It cannot purely however be
be represented that, in point of fact, the change of treatment of pneumonia led to the same change in treating all other acute inflammations, and still less all fevers. I can bear witness at all events that the abandonment of bleeding in idiopathic fevers preceded by a good many years in this city, its abandonment in acute inflammations, and that its future abandonment in the latter took place simultaneously in all acute inflammations, and not, as Dr. Bennet urges as to pneumonia, because of an improved diagnosis, for there are several internal inflammations whose diagnosis did not make any sensible progress either immediately before or during the change in their treatment. Professor Bennet refers to some Indian physicians, Boulland and others in Paris as having recognised no alteration in the type whatever, yet surely the late professor Alicion, Professor Christian and others are as discerning and as worthy of our confidence as those who support Dr. Bennet in his views. The last authorities mentioned had besides actual experience of the change, and even noted its progress which unfortunately the last mentioned was not privileged to do. Nor can
we suppose, that those great advocates for the change of type, would be likely to conform to the improvements in diagnosis and their results with the progressive change in disease as they observed. Of during the epidemics of fever between the years 1817 and 1820, Dr. Christie had so far observed the character of relapsing fever as to recognize it as the dyspepsia of Corren, and to delineate it distinctly as it then occurred, and also to recognize it as it appeared in epidemics subsequent to 1827. Though the intensity which it had manifested previous to that date, surely a point is gained towards establishing the theory of change of type in inflammations as well as fevers, constitutional points of view. Dr. Bennett cannot doubt, that, in the epidemic during which he had attacked of the dyspepsia, the enormous quantities of blood that used to be taken, not only with impunity, but with a beneficial result, could not have been borne. And in truth he admits that "Changes in diet, in locality, in climate, in atmospheric influences and a variety of causes may induce strange modifications in fever"—it is but going a step further to admit that these influences, or others less apparent, and
and Hitherto undetermined have induced an attitude in the character of inflammatory diseases.

But Dr. Bennett, in the proposition quoted at page 13, denies the analogy between fever and inflammation in respect to changes—whether from the greater difficulty of proving the latter by assigning a sufficient reason for the change, I know not. Still, the mere ignorance of such reason cannot really affect the fact any more than other unknown causes can affect their known effects. Yet this seems to be the tendency of his argument. Many have made vague suggestions as to the probable cause of the change—such as the visitation of the Cholera and introduction of railways, but it may be admitted, without conceding much that these at best are but vague suggestions. However, Dr. Bennett might find it as difficult to assign a reason for many other phenomena—for example, the origin of such diseases as are not only propagated by direct infection or contagious typhus syphilis. That there must have been a first case of such, (to instance one of the most communicable disease of hence, but what were the circumstances under which it occurred? Did it arise from pure tillure?}
tillicic, atmospheric or other influences external to the individual? DuChâtellet would seem to prove that, in circumstances most favourable for this origin, it does not arise in this way, and may be of his opinion (Chisolm, Perciffo, &c., &c.). Now if this could not thus make it appear, the only other way in which it would seem possible for it to have originated, would be from some peculiar condition in the human constitution, not previously existing, affecting all races, and entailing on the successors of people then alive an everlasting liability to attacks of the disease. Of course this vague conjecture might be extended to many other diseases and might, so far perhaps, help to explain the recent change of type of inflammation, although the cause of that peculiar condition or even the condition itself, is beyond my power of elucidation.

Again with regard to the abandonment of blood letting by Professor Bennett in all cases of pneumonia, I intend making a few remarks. Though his present treatment of pneumonia has been wonderfully successful, yet pointing that something is due to improved means and greater proficiency
in diagnosis than in former times, it might not prove quite so successful in a reappearance of the chronic inflammations. And granting also that "formely, bleeding was not practised in many cases where pneumonia was present, whilst it was largely resorted to in those where that disease never struck at all"—this is by no means sufficient to warrant anything like a condemnation of the remedy in all the cases to which this treatment was had recourse to. It seems quite possible to me at least, while reasoning on the matter, that even now cases may be met with occasionally [these cases may indeed be rare] in which, from the extent of the lungs involved and therefore from defective aeration of the blood, bleeding may be indicated. Of course it may be objected that these are too much probabilities as almost the called impossibility, seeing that, in phthisic, as much as from fifth of the lung might be disorganized without causing any discomfort from imperfect aeration. But then in such phthisiacal cases, I would suppose that the amount of blood requiring aeration will decrease, pain pass, with the destruction of the aeraating organs; and indeed
This seems likely enough from the atrophy and malnutrition of all parts of the body. And by having recourse to bloodletting in cases where a great amount of inflammation existed involving intense dyspnoea, we would only be seeking to remove the immediate danger without perhaps at all checking the course of the real disease. And further, we would only imitate nature in the relief she affords to the consumption. Now this does not at all advocate the abstraction of blood in such a way, or for such a purpose as Professor Bennett seems objects to. It would not be an empiric practice at all as are those condemned by him;

the diminution of the amount of circulating fluid in order that 1° the materiæ motæ in the blood would be diminished, 2° less blood would flow to the inflamed parts 3° the increased quantity of blood would be lessened. Indeed Professor Bennett himself grants that it may be useful in "certain cases of bronchitis presenting aeration," and if it be true as Dr. Watson says it is, that "pneumonia without bronchitis is rare," Professor Bennett does not absolutely exclude bloodletting from...
his other remedies even in cases of pneumonia—
complicated of course with bronchitis.
Watson says "Delirium is a symptom which
very frequently occurs in the course of an attack of
pneumonia; and a very ugly symptom it is. It
denotes that due arterialization of the blood is largely
interfered with by the pulmonary affection. It measures
in one sense the quantity of mischief which is going on
within the thorax; and it is a direct evidence that
the pectoral mischief is telling through the circulation
of venous blood upon the brain." How if the "due
arterialization of the blood" is so far interfered with
as to produce such an "ugly symptom" as Watson
calls it, why may bloodletting not be had recourse
to in this case in order to restore equilibrium between
the amount of blood requiringuration and the
partially disabled aching organs?
Perhaps also it might be found necessary and to
produce a good effect when in the country this
disease has attacked arduous farm labourers. In
such case nothing is more likely than the disease
will be aggravated by the impurity of the air around
the patient from deficient ventilation of the house.
Such
Such accidents, however, seldom, if ever, encountered in higher or in hospital practice, where a good atmosphere is always at the command of the patient or physician.

Physiognomical diagnosis is a subject which has recently been brought before the notice of the student in a systematic and scientific manner by Professor Laycock. During the course of the present session he has delivered a series of lectures on this subject, in which he describes four great physiognomical types to which all others are reducible directly or as combinations of two or more.

He showed, by these, the particular system of each type which was especially liable to disease, pointed out the influence which the diseases of any type was likely to exert over other diseases in the individual, the circumstances originating or modifying the peculiar tendency of each type to its disease, and the importance of these tendencies in the treatment of disease generally.

Formerly, and in the absence of such means of diagnosis as the physician now possesses, it must have been of very great importance, and it still is.
claims no inconceivable share of attention whether it receive it or not. But in fact few professionals seem actually willing to relinquish it altogether for the more exact means of diagnosis, although they may nominally or professedly reject it. The word 'diagnosis', cachexia referring as they do to physiognomical diagnosis prove that it is not altogether unimportant or unnoticed. Nor ought it to be so. No means which are capable of rendering any assistance in arriving at a correct diagnosis ought to be neglected. It cannot at this time with propriety be classed among the things that were for it may still prove of really practical value either as an auxiliary in diagnosis or as indicating along with other phenomena, the nature of the treatment best suited to a particular case. Indeed it has, to me capable in some instances of occupying one of the most important positions as a means of diagnosis in cases of anemia. Chlorosis gives a certain disease, and I believe that there are many circumstances in which it is more nominally than really undervalued. I say more nominally than really, because in such cases it is made use of as
it is apt to be, instinctively, by the physician and may thus aid him, though unconscious of the fact, in forming his conclusion with regard to the character of a disease. It is not infallible, but as much may be said of almost any means to which we can have recourse. Nor can it be said to have been brought by professor Laennec to be brought too prominently into notice, for though its part is merely auxiliary or corroborative it is still important. It is really least valued by those who pay least attention to it—the reason of this being evident enough, they are unaware of its value, or they are so, just because of the little attention they have given it. Generally speaking it would seem that it has not received a proper share of the regular and scientific attention which has been bestowed on other subjects that too little inquiry has been made into its merits or demerits, each practitioner being content with that acquaintance which casual observation may afford or which is only instinctively acquired. But this is not the way to advance an element in any science. Had other means of diagnosis been treated in a similar manner what position would the science of medicine have been in as regards the diagnosis of diseases? It may be said that
that it was less worthy of notice than other means and therefore had not an equal share of attention. This can hardly be considered a satisfactory explanation, perhaps a more probable is that the neglect is due to the fact of its being less palpable and more instinctive means than others which we have now. Yet which of the latter could have alone accomplished as much as the history of the science of medicine informs us was accomplished by the physiognomical diagnosis? The wonder is not that so little, but that so much was done by this means. That it might have remained a source of great interest and importance is very probable had the corroboration of the phenomena it was alleged to indicate been sought for in the hereditary histories of patients, in their personal histories, or in the results of examinations after death. Morbid anatomy has done much to make other means of diagnosis not exact, and why should physiognomical diagnosis have been so much excluded from association with that which might have rendered it proportionally exact? With regard to its exactness I have already said that it is not infallible, but still I think I have shown it is able, by it, to discover by its tendency, to disease which
which were corroborated by subsequent attacks of the
disease, nor am I willing to admit that on such
occasions I was only fortunate in guessing, or to at
tribute all to contingency. An eminent physician
in Edinburgh who is disposed to pay little attention
to this subject says of diathesis that it is the incipient
beginning of disease — if so, could such very incipient
beginning be ascertained by any other means than
the physiognomical?

But one thing remarkable about this is that those
who can discover tendencies in this way have not en
soured to act on the principle — veniunt secun et
morire. It is perhaps equally surprising that those
who say diathesis is merely the very incipient begin-
ing of disease should have paid no attention to this
throughout; they have refused to check the disease in that stage of its
progress at which it is most amenable to treatment.
Of course the one party might argue that an inci
tendency to, and the other that a scarcely perceptible
stage of disease are what the subjects of them don’t
advised about. Besides, the advocate for mere manifesta-
tion of tendency by diathesis say that the develop-
ment of such tendency into actual disease is de-
}
Frequent on circumstances, that circumstance which m
one case might develop a diathesis into tuberculosis, the d
acid might develop another diathesis into acute rheumatis-
ism. Still, maxぬぬl of professor Laycock teaches that di-
reason - this is something, and may serve to explain the
old adage "what is good for one is bad for another." Nor
is this all, according to professor Laycock, as the treat-
ment of disease we may be able to guard against mis-
takes in the regimen and administration of medicines
which are liable, though rarely, to result from idio-
ysis. He has for a great number of years paid
particular attention to this subject in every respect,
and finds that, in the very great majority of cases
such idiosyncrasies are met with in persons of the
composite physiognomical types - the neuro-arthritis.

However, were the attention of the profession directed
generally to this subject, it is possible that something
might be found in it worthy of acceptance, which
might rescue the physiognomical diagnosis from com-
iservative disdain, and lead to the acknowledgement
that it still is as convenient and interesting and as
worthy of cultivation as any other at our disposal.
Acute Rheumatic is a disease which has been and is dreaded, not so much perhaps on its own account as on account of what are considered frequent and terrible complications over which the science of medicine can exert little or no control. "Patience and long weeks" may carry a patient through the disease itself and still recovery from those complications be impossible and a fatal termination not far off. But if we regard Rheumatic endocarditis as one of the phenomena of acute Rheumatic then of course we would only be entitled to say of such cases that recovery had been made from the more apparent phenomena - the more insidious and dangerous phenomena remaining from which recovery, as yet impossible. My reasons for supposing that it is as much a part of the acute Rheumatism as the affections of the joints or are follow.

The peri and endocardium are derived primarily from the same embryonic membrane as the serous membrane of the joints which are regarded as the peculiar seat of this disease. The perichondral and cranial cavities the cavities of the thorax, abdomen and pelvis all lined with serous membranes are melowers referred off.
off from the general cavity of the praeordial sero-

layer of the valve. The limbs with their joints and

their fibro-serous linings are also derived from the same

membrane. How then could I establish any argument

on this? Must I concede then why the endocardium

and epicardium are of all other serous membranes

those lining the joints excepted, particularly liable
to be involved. This I shall endeavor to do.

The peri and endocardium have something in

common with the serous linings of the joints, which

is required of the other serous membranes to a much

less extent. The serous membranes of joints lubricate

surfaces which, owing the functions of joints, are

made to rub against one another frequently,

t. sometimes for a long time. We have also, owing to

the movements of the heart, its membranes subject
to friction of the same kind, but uninterrupted, t.
during a febrile attack especially, active friction.

The nature of this friction is not irritating under

ordinary circumstances, owing to the fact that the

surfaces are well lubricated. Yet the fact that it

is continuous and active during all times and, specially

active during a febrile attack, which is highly disagree
to influence the membranes subjected to friction in some way, seem to show why the membranes of the heart are more frequently involved than the others referred to. If we take into account also the fact that when the membranes of the joints of the extremities are affected, motion can be thoroughly restrained and so friction of these membranes when irritated is prevented, while the action of the heart is not only undiminished but increased, then the friction of its membranes also increased, we may observe something like a probable reason why the joints are frequently left as well after the disease has gone by, as before its appearance, while the heart never recovers from lesions which affect it during the progress of the disease.

20. If the cardiac membranes are not invariably affected in acute Rheumatism, they are often enough so to allow of our deeming their affection as a veritable part of the disease as much apart of it as the affection of the joints are. The heart was found involved by Bouilland in 119 out of 246 cases by Budde in 24 out of 48 by Latthein in 74 out of 130 by Taylor in 27 out of 49

So Total 241 out of 479
So that by these statistics it would seem that the heart is involved in about one in every two cases. Now I would have considered myself fortunate had I been able to discover that there were any particular joints liable to be attacked more frequently than one out of two cases. I have however only been able to ascertain that the knees, ankles and wrists are the joints most frequently affected, without having been able to arrive at the relative frequency, and it must remain with those who have opportunities of observing these cases to make determined their proportion of particular joints affected in particular cases. But whether this ever be determined or not is of comparatively little moment. I am inclined to think that but few really typical cases of any disease are ever met with that the physician expects to find only a number of but not all the conditions of any disease on which to form his diagnosis, and early this principle might be extended to the subject in hand. Dr. Watson considers it "a curious and instinctive circumstance that rheumatic carditis is something the first step in the whole disease." He further says, "The cardiac symptoms do sometimes, I mean, precede those of the joints; even by two or three days." I affirm that
that this circumstance can only be curious and un-
structure into for as it proves that the Rheumatic
Carditis is not always the result of metastasis—
it may be that it is so, because it shows that it
is as much entitled to be regarded as primary
though not necessarily affection as the affections
of the joints are themselves.

From all that I have able to learn acute rheuma-
tatia is a blood disease — that there is a materia
morbis in the blood giving rise to it, for which
materiee morbii the leucocytes tissues have a firm
electric affinity, or which acts on those tissues as
a specific irritant, much in the same way as certain
medicines act on particular organs or tissues, e.g.

diphenic acid and strychnine — that the
composition of this materiae morbii is never to be
determined though its character is that of acids.

Dr. Prout, Lee and others say it is lactic acid, and
Richardson, supported by experiment, seems to favor
their view. Virchow, on the other hand, bas on what
Richardson’s experiments, has been unable to
come to the same conclusion, and Dr. Pennie again
thinks that the particular materiae morbii is lactic
acid.
acid. Other theories have been formed and combated, but I think that, as I have already remarked, no thoroughly reliable conclusion has been come to by those who have investigated this matter. On the whole, however, I am disposed to favour the opinion that lactic acid is the cause for the following reasons:

1. Lactic acid exists normally in the gastric juice.
2. It is eliminated normally by the skin.
3. Cold and damp, which check fermentation and elimination by the skin, have been regarded as indirect causes of the disease in those of the chronic.
4. Acid indigestion precedes an attack, as if the acid existed in abnormal quantity in the stomach owing to the elimination by the skin being prevented.
5. Much lactic acid is eliminated by the skin during an attack of the disease.

I hardly know whether to think these reasons are sufficient to entitle me to conclude in favour of Richardson's view but I am content with them at present until I find better reasons in support of another. But now for the treatment of this disease - what of it?

Before saying anything on this important subject
I would offer a few remarks on an important character of the disease. All authors on acute inflammation take particular notice of the liability to leave one part and go to another. "This tendency to shift its place—to what is usually called metastasis—is a very remarkable feature of the disease. The inflammation will appear at one joint suddenly and as suddenly subside in another which it has occupied; and then perhaps it will jump back again to its old grantees." How had Dr. Watson kept what he had said a little before about the joints and "the perpetually moving heart," he might perhaps have discovered something which could account for this tendency to metastasis.

1. That there is an elective affinity of the part of the fibres across membranes for the matters morbi (or, if this be not so, the matters morbi must exert a specific irritant action on these membranes).
2. We have this affinity (or irritant action) aided in determining the affected joint by the irritation of friction caused by motion already alluded.
The determination of blood (containing the maddness morte if must be remembered) in great relation quantity to the joint moved, owing to the exercise of the function of the joint. Now if any joint in a limb be affected, the patient giving perfect rest but will continue to use the unaffected limbs, each joint of these being subject to precisely the same influences across the affected joint before it became rest of the disease. Other circumstances being equal with the joints, but rest being allowed one by another subjected to motion the metastasis could be determined from the former to the latter. This explanation of metastasis — if it be the correct one — must be of considerable importance in reference to the treatment of the disease. It would seem by it that there is a limit to the local phenomenon more or less extensive according to the prevalence of the attack; otherwise, why should it leave one joint to attack another? I do not know of any reason for this limitation yet further investigation might afford an explanation and establish the theory. And if on further investi- gation this was ascertained to be correct, then an attempt might be made to direct as it were the disease from the
the heart by causing it to spread itself to the utmost on the joints. One way perhaps of accomplishing this would be by enjoining firm and active use of all the unaffected joints, and by retaining it in all the joints which are affected until the disease has yielded to time or treatment. If this succeeded in fixing the disease in all the joints of the extremities, this would certainly be a distressing state of matters; but if this means the disease could be prevented from lying on the heart, surely the result would com: 
1"jenseit for the greatness of the suffering.
My object in the remarks that follow is not to criticize the treatment generally followed now, but to show how some (and perhaps the best) methods serve to corroborate the preceding view, as well as to suggest what seems to me a slight improvement in the applica: 
33.3.3tions of remedies for acute rheumatism.
Alkalis have been recommended and used successfu: 
1. Garrod and Golding Bird (the bicarbonate of potash by the former or the acetate of potash by the latter. 
By these, two indications are evidently fulfilled. 
10. The acid (muriatic miscible) is counteracted and 
20. Owing to the decretic properties of these alkalii, the
The elimination of the matter most by the kidneys is retained.

Dovers powder is not now so frequently used as it has been and this may be owing to the improper administration of it formerly, too much being expected from it. It is rather remarkable that when nature is evidently struggling to get rid of a materia morbid, and succeeding admirably, she should be called on to effect more that can with impunity be attempted. Had Dover's powder been used only as a gentle diaphoretic, in acute pneumonia, to nature when not acting efficiently, and not as an astringent when doing well, it might even now perhaps have been regarded as a valuable medicine in this disease. Had it been given so as to induce or keep up gentle diaphoresis when the skin was dry, and not as a stock remedy, irrespective of the state of the patient, it might have afforded some evidence of virtue in the disease. But in this as in some other instances in which certain medicines are looked on as stock or routine remedies for disease, the failure is frequently attributable to the physician's prescribing, and not to the medicine itself.
Opium has proved useful as far as regards its analgetic properties, but it would perhaps be difficult to find a case clearly proving that it is of service otherwise—i.e., in checking the disease or averting its dangerous tendencies.

Aconite had been lately approved of both as a local application and an internal medicine, and, from the views expressed in the preceding pages, I am disposed to attribute its most successful administration to its sedative action on the heart. Accordingly, were it still more extensively used, merely to keep up for a considerable time a slight sedative action by which the motion of the heart and consequent friction of its fibroserous membranes would be diminished and used also with those medicines already noticed which serve to counteract or eliminate the medical morbid, perhaps more favourable results than with the might be attained. The heart, according to the views I have expressed, would be so far protected by this treatment of the disease cut short by the removal of its causes.

With regard to the local treatment, that is, the treatment of the affected joints, I have already alluded
alluded. In endeavouring to palliate the suffering I would not seek to bury the disease from any part only have recourse to such means as might palliate without incurring the smallest risk of metastasis and indeed endeavour to retain the manifestation of the disease in the least dangerous localities.

The dry treatment (retaining the points in warm flannel or cotton wadding) seems simplest and the most innocent of all local applications, and may relieve pain and protect the part without causing it to change its site to that which I would at present be most disposed to recommend.

Before entering on a course of study in medicine I had seen a second attack of smallpox terminate fatally, and I remember being much impressed with the idea that a second attack was not impossible. The case was that of a man in the prime of life, who previous to the attack which ended fatally, gave evidence by his face of the severity of the disease when he first suffered from it. Since I commenced this thesis I have not been frequently thought of this matter and have tried to discover the probable cause of a recurrence of such
such disease.

No one doubts the efficacy of vaccinia as protection to a certain extent against smallpox, yet there is a difficulty in determining to what extent it is a protection or how long its protective influence may last, nor would it be readily believed that it is prophylactic in the same degree as an attack of the disease itself. While reasoning on this matter, I have encountered some difficulties which I cannot say I have succeeded in overcoming, and in the reflections that follow I must ask to be excused for vague and inexpressive which I could not obviate.

Pregnant females have given birth to children which have apparently died from attacks of this disease while in utero. Children have also been born alive with the smallpox eruption on them. In many of these cases the mother have not been afflicted with the disease. From these facts it would seem that the blood of the mother must have conveyed the poison to the fetus, it being the only (even indirect) communication between the maternal poison and the child. I account for the mother of
A ferment capable of acting exclusively on a fermentable body (without involving in the fermentation following it) is a very rare, but the latter I find that something like this view has already been expressed.
being affected although the matter morbi (3) was in her own blood, I supposed, that really the matter morbi of smallpox consists of two parts, one of which is incapable of causing disease, the other be absent; that one of these exists in the blood of those capable of infecting and that the other is the part is conveyed by the atmosphere into the system, that these both combine to produce those phenomena which constitute the disease itself, that during the disease there is a reciprocal action by which the precipitating agent or condition in the blood is destroyed, the disease brought therefore to a termination. Now if by a previous attack or by artificial means (vaccination) the condition or ingredient peculiar to the blood be absent from the blood of the mother, it seems quite reasonable to suppose that the atmospheric part of the matter morbi may without inducing disease in the mother, be conveyed by her blood to the fetus and cause disease in it.

Miss Nightingale in her book on nursing says that smallpox follows, or may be caused by other diseases, often and more reliable authors on
on this subject are not unwilling to admit, and I have seen one case in which this disease seemed as it were to tread upon the heels of death. From all this I am disposed to believe that the changes produced in the blood and system may be occasionally precipitated as an agent here, capable of acting the part of the atmospheric constituent of the material morbid. Even after the occurrence of an attack of small pox, and the complete destruction of the condition or ingredient in the blood suddenly, individuals susceptible of being affected, it is not improbable that it may be revived and again induce a susceptibility to the disease. The reason that I can find for the restoration of the state of matter is in the physiological change which occur at different periods of life, not pathological change produced the same result, and the occurrence of one or more of such changes during the interval of the disease in this way the revival of the condition may occur in one and not in another, can hardly be opposed to this view we bear in mind that these changes do not go on
to the same extent, or in the same proportion in all cases.

With regard to the action of the vaccine virus as a prophylactic, in conformity with the ideas just expressed, it may be regarded as destroying or modifying the character of the part of the materials mobiled culinar to the blood.

I intended to have continued my observations further on other subjects, but "this being," and believing that, with our teachers, the object of a thesis is mainly to ascertain whether or not the student is deserving of the honour to which he aspires. I hope the preceding pages will be found sufficient. I have endeavoured, in advancing arguments for or against any particular theory, carefully to consider all circumstances and to decide impartially. And as I think it becoming for any one to entertain an opinion or express a sentiment on any subject with too great reference to others (much wider and of vastly more experience than himself), and with too little regard to his own convictions, I trust that my candor will
will not be construed as presumption and dogmatism. I hope I have not too much partiality to my own views on any subject, and believe I would be able to adopt those of others without hesitation as soon as I am persuaded that my own are inconsistent with the truth. Yet I am unwilling to admit that I would even orally relinquish them so long as I believe them to be correct. I however confess that I owe much to the kindness and instructions of my teachers, and trust that though I differ in opinion in some respects from some of them they will give me credit for this—that in differing I do so candidly and at the same time without compromising in any degree the respect which I entertain for all. I claim besides for myself the liberty to think and to argue freely according to conscience.