On
Some of the actions and uses of
Water as a
Therapeutic agent
by
William Meikle
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Water is one of the few articles in the Pharmacopeia of all nations. Its constant employment in health as the means of allaying thirst must have suggested to the very first of our race who in disease suffered from that denation in excess, its use as a remedial agent, and from that time to this and in all quarters of the globe, water has either been, or as a vehicle for the administration of other remedies, occupied an important place in the materia medica.

Its history as a Therapeutic agent, like that of every other remedy of any standing, has been marked by the ebbs and flows of fashion. These however have left its reputation intact, and today it occupies as high a place in Therapeutics as ever it did.

But while this is true, it is also true that notwithstanding its widely spread and long continued employment as a curative agent, and notwithstanding the value of some of the Therapeutic indications served by it, the rationale of its actions and the principles which regulate its administration,
have not yet been fully investigated. Nor need we be surprised
at this, for however long, however widely a remedy may
have been employed, the truth regarding its actions
and uses is discovered only in proportion to the advance
that is made towards truths in physiology and pathology.
And since these sciences are but the growth of little more
than the past half century, we need not wonder to find
that in regard even to such an apparently simple thera-
petic agent as water, much remains yet unexplained.
Perhaps too and the reason for this to be found in the
very fact of its apparent simplicity. The tendency of
the human mind to prefer the complex and the subtle
to the simple and the obvious has almost as many illustra-
tions in scientific as in common life. But whether we have
such an illustration or do not stay now to inquire,
but proceed at once to the task we have assigned ourselves.
And that task is not the clearing off of the cloud which still
hangs around many of the therapeutic actions and uses
of water, but that we proceed is merely to state a very
few of these actions and uses, and to explain them
according to modern physiology and pathology, so far
as we are acquainted with these.

In investigating the therapeutic actions of any substance
it is of course essential that we know whether the substance
in question is being employed per se or in combination with
other substances. All such investigations are from their
very nature surrounded with serious difficulties, but there
are increased tenfold if we are ignorant of the composition
of the remedial agent under examination. Here according
we might enter into the chemical history of water together
with its adulterations and their tests. This however would
be altogether foreign to our purpose. We shall therefore suppo-
tent these steps have been taken, and that the substance
with which we have to deal is chemically H2O, and pharma-
centrically, aqua pura. Next when this is done, are we
entitled to look upon water as a simple substance? We
think we are not. It is true that so far as pharmacoeutic
chemistry is concerned, there is nothing more to be detected.
but the sooner it is subjected to physical examination
than it is all the apparent that beside H2O, water also
contains in varying quantity one of the most important of the
imponderables viz. Caloric. Of this there is evidence in
such abundance that in place of stating it, it becomes ne-
rater to state why we mention the fact at all. We have
done so, not because we think there is anything in the least
deprecated in the circumstance that water contains
Caloric—a substance so far as we know, universally
diffused, and therefore not at all likely to be absent
from water, but because as we shall see presently;
it is of importance to distinguish between the therapeutic
actions of water simply as H2O, and its actions when com-
bined in various proportions with Caloric; and employed as
a medium for the abstraction or application of that agent.

What then are the therapeutic actions of water per se
and what as a medium for the abstraction or application
of Caloric?
Neither of these questions would we now attempt to answer, with any degree of fullness, and since it is the latter which we mean more particularly to consider we shall here notice the former very briefly.

The therapeutic actions of water as \( \text{H}_2\text{O} \) observed chiefly on its administration internally, are numerous and important. That this should be the case would seem a priori not improbable from the fact stated by Milller that few-fifths of the weight of the animal tissues consist of water and that without some such proportion their healthy action is impossible. The introduction and exertion in larger quantity than normal of a substance playing a part so important in the organism, can hardly fail to exert a powerful influence on the action as well as on the composition of that organism. Accordingly we find that in many morbid states Nature indicates the employment of water internally by the existence of intense thirst. This occurs either when the loss of fluid by evaporation or otherwise has been excessive or when some aerial substance, whether introduced from without or merely an altered secretion of the alimentary canal, has been causing irritation. In either case the water acts as a diluent. Dilution however in such circumstances cannot be regarded as merely mechanical. The water is absorbed, and a portion of it at least combining chemically with those forms pre-eminent part of the tissues of the body. Hence however it can exist only in certain proportions for whereas there are excreted those organs whose special
function it is to regulate the accounct of fluid in the body are called into more than usual activity, and thus we have water acting as a diuretic, a dephlogistic, a seroserific, and a purgative or rather a laxative.

In this, the hasty sketch, we do not stop to mention the various morbid conditions in which these different actions of water as it is indicated, but proceed to notice another of its actions, which we think is of hardly less importance but which is not so often referred to by writers as its action as an alterative. We are not attempting here that this term quite expresses what we mean, but it comes nearer to it than any other therapeutic term with which we are acquainted, and since all such terms are but rough approximations at the best it may serve our purpose sufficiently well.

The action which we would thus designate alterative is that in virtue of which water taken internally in large quantity than thirst and necessary exertion demand, hastens the natural metamorphosis of the tissues. That such should be its action, we would, keeping in view the proportion of water to the solid elements of the body also referred to, have a priori expected. For if water is being imbited and exerted in unusual quantity, we would naturally, conclude that so far as four-fifties by weight of the tissues of the body are concerned, transformation is going on with unusual rapidity, and owing to the intimate union subsisting between the fluid and the solid elements of the tissues, it would hardly seem too much to
Suppose that the same might be true of the remaining fifth.

On this point however we are not left to speculate for that such is the action of water has been distinctly proved by the carefully performed experiments of Dr. Böckler. We cannot now enter into the details of these, but shall content ourselves with a statement of the general conclusions to which his observations lead. Before doing so however it is necessary to mention that these conclusions are drawn from two sets of experiments each of which occupied a period of seven days. His food and exercise were throughout both the same, while in the first he drank about two pints of water daily and in the second about seven pints. Hence concludes Dr. Böckler:

1. "That water increases interstitial metamorphosis of the tissue, and consequent loss of weight."
2. "That the decomposed tissue is excreted partly by the urine and partly in solid faeces."
3. "That the water formed in the organism by the change of the tissue is augmented, as well as the nitrogenous constituents of the secretions."
4. "That the excretion of carbon by the lungs, and the quickness of the pulse and of respiration are not affected."
5. "The necessity for food keeps pace with the metamorphosis of tissue."

Now in the words of Dr. Böckler's reviewer: "Metamorphosis of tissue is life, or an inseparable part of life, and there is reason to believe that when it goes on quickly and there is the possibility of a supply of new matter equal to the exhaustion,
of the old, the tissue changed is in a more perfect state, more able to resist external noxious influences and therefore more likely to last long than when exchange is slow. This rapid metamorphosis of the tissues then, the copious inhibition of water does, under the conditions observed by Dr. Stocker certainly affects. Now it would be strange indeed if a physiological action of such importance were incapable of being turned to any account therapeutically. But in order to assure ourselves that such an anomaly does exist, we can only to inquire whether there exist morbid conditions, marked by an abnormally slow transformation of tissue, to recur an answer in the affirmative from innumerable cachectic condition and chronic diseases; while proof of its correctness is to be found in the anaemia, the mal-assimilation and the sluggish or defective excretion which characterize so many of these conditions. This might settle the question regarding the possibility of any such therapeutic action as that of which we are speaking, but we have still further to inquire whether the copious inhibition of water, with of course those precautions which an examination of each individual case would suggest to any medical man who had any experience of the remedy, had in such cases been followed by the therapeutic effects which its physiological action would lead us to expect? So far as we have been able to ascertain there are no facts recorded which would warrant us without hesitation to answer this inquiry in the affirmative. We hesitate not because there are not cases belonging to this class
of which we are speaking fully and carefully recorded in
which the copious imbibition of water has been employed
and care has followed, but because it is seldom that
such cases are under treatment at all without other
remedial means being at the same time employed, so
the result has rendered complex.

Perhaps one of the nearest approaches we have to the
treatment of chronic diseases and cachectic conditions, by
the copious imbibition of water, alone, is to be found in the
employment of mineral waters, of which we cannot but
think that the efficacy in many cases depends much
more on the aqueous than on the mineral ingredients.

It is difficult however from existing facts to arrive at any
satisfactory conclusions on the point, but we think something
like the following may be regarded as near the truth: viz.
that while the morbid, slow transformation of the tissues
which forms the great feature common to so many diseased
conditions, is benefitted by the copious imbibition of water,
there is something specific characterizing each of the
various groups of these conditions, which renders that
group capable of deriving more benefit from one mineral
suppuration than from another.

The only other circumstances with which we are acquainted
in which the copious imbibition of water is employed are
an alteration in in the course of hydropathic treatment,
and here we find the same difficulties in drawing a
decided conclusion on the point in question, for the various
other processes to which the patient is subjected simul-
with its application of water, cannot but be regarded as at least as little inest as the minute mineral impregnation, of many spasms, and consequently tending in the same degree to complicate the results. Here however as in the case of mineral waters, since it cannot be reasonably doubted that, under such treatment, many enmeshed conditions of the nature we have indicated are cured alleviated, we seem to be warranted to regard as an approximation to the truth the general statement; that while the copious application of water meets the alternative indication common to so many diseases, the various other hydrostatic applications, skillfully employed, may be made to serve such indications as are peculiar to each individual case.

Such then is the evidence we have to adduce in favour of the supposed alternative action of water as H2O. Nothing could be more satisfactory than the testimony of physiolog; but we regret that we can discover nothing more conclusive than the above in the records of medicine, regarding the therapeutics of the question. We trust however that here as elsewhere in rational medicine therapeutics will ever lay down the road of physiology, and indubitable evidence be afforded either for or against the employment of H2O as an alternative.

Having thus glanced at some of the therapeutic actions of water as H2O taken internally, we might now have spoken of its actions when applied externally. We would, however, only venture in passing that thus employed its therapeutic properties would seem to depend on the mechanism...
excluding the atmosphere and partly on the strong affinity, which the tissues have for H₂O, which being thus absorbed rests the cutaneous surface, and in many diseased conditions of that surface tends to promote the restoration of its function.

Leaving now the therapeutic actions and use of water as such, we go on to notice some of its actions and uses as a means for the abstraction or application of caloric. In this capacity, we find that water is employed both internally and externally.

Internally, it possesses those various properties of which we have been speaking, but these are very considerably modified by the quantity of heat combined with it. Thus it may be stated generally that between the degrees of 130° and 150° Fah. it acts as a digestive stimulant and sudorific, about 100° or 110° it acts as an emetic, while between 32° and 60° its operation is that of a refrigerant, a tonic, a diuretic, and a sedative.

Of these we must content ourselves with the mere enumeration and proceed to consider as the chief part of what still lies before us, some of the actions and uses of water as a medium for the abstraction or application of heat employed internally.

Here as or in its administration internally, we find its action varying much according to the quantity of caloric with which it is combined, not to mention how the various other circumstances, which concur to regulate its action. Thus water applied externally, combined with a large quantity of
Caloric, 110° to 212° or more, according to the quantity, acts as a stimulant, a rubefacient or a reluctor; with a smaller quantity, 80° to 98° its action is calming, while with a smaller quantity still, 33° to 60° it acts as a sedative or a tonic.

An examination of the whole or even of the majority of these actions would far exceed our limits. It being therefore necessary that we make a selection we have chosen for consideration the two allied actions of calming and sedation.

Here then we have first, to settle the sense in which we understand these terms, to be applicable to the therapeutic action of which we have now to speak. By the calming action of amniony, then we mean that action in virtue of which morbid excitability of the nervous system is allayed, while by its sedative action we understand that in virtue of which it allays morbid excitability of the muscular system. Now since the two morbid conditions thus severally indicating the one the sedative and the other the calming action of water, or other medicinal agents are in nature in almost every case more or less blended, it is obvious that a rigid adherence to any therapeutic distinction founded on these morbid conditions is impossible. Instead therefore of what follows of fettering ourselves with the vain attempt to discuss, first the one action and then the other, we shall consider separately three of those modes of employing water in which both actions appear to be to a greater or less extent secured, though at the same time often therapeutic actions act in the causes or
as the effects of these are also developed. The three to which we refer are cold affusion, the warm bath, and the wet-sheet packing. In the first the sedative action may be said to predominate, in the second the Calmative while in the last the two are combined - the sedative shading off into the Calmative.

I. Cold Affusion. In this process some gallons, more or fewer according to circumstances, of water at any temperature between 33° and 60° are poured over the naked body of the patient from a height varying also according to circumstances. The patient is then thoroughly dazed and thereafter returns to bed to go off to take some active exercise.

We are not aware that any experiments have been performed to determine with accuracy the physiological action of this process, but it is obvious that such a mode of employing water as this may be made to vary in its physiological and also in its therapeutic action to a very great extent. Thus if the pouring be continued for only a very few seconds we would expect in ordinary circumstances a tonic effect to be produced, especially if the water were made to fall from a considerable height; if on the other hand the affusion be continued for some minutes without more than a few inches of fall we would expect the action exhibits to be directly sedative. The ground of our expectation in the latter case would be the well-known effects of the abstraction of heat by any medium - a short abstraction acting as a tonic while the same or a similar process, lengthened produces a sedative effect.

The peculiar properties of water as a medium and of
cold applications as the mode of employing it are insufficient, plain. Thus the capacity of water for heat, exceeding as it is, well known to be, that of any other substance, yet, it is a remarkable manner for the purpose for which cold application is employed — the abstraction of heat. For while its effects depend, in whatever way we modify it, on the abstraction of heat, by cold application we have the power, as already indicated, of producing at will a tonic or a sedative action. Then the former action is indicated when heat is not abstracted in large quantities, or if it is, its speedy redissipation is favoured by the shock of the weight of the falling column of water. Then known is the latter action is sought, a large quantity of heat is abstracted and that in such a way as not to arouse the system to put forth any effort for its redevelopment. But while in ordinary circumstances, the tonic and the sedative actions of water seem to be thus produced, we find here as in regard to many other tonic remedies, that in certain diseased conditions they act as sedatives. Of this we shall have abundant evidence as we proceed to inquire in what morbid conditions the cold application is indicated.

We have not far to seek for a reply. A process whose action depends upon its power of abstracting heat must, in order to act usefully, be morbidly high degree of heat, upon which to operate. "Essentia vero febrorum est fracta nativae caliditas." It is then, whether we adopt this definition of fevers or not, in the febrile condition, that cold application is indicated, such at least is the conclusion to which the
preceding considerations naturally lead. If however this conclusion had had nothing more than these on which to rest, if we had been unable to adduce facts in its support we would not have troubled ourselves with the therapeutic action of cold affusion at all. More than one are it to look for some of these facts?

The question suggests to every one the name and experience of Dr. Eancer of Liverpool. For although that gentleman was not the first in modern times to employ cold affusion in fever, it was by the energy, ability and candor with which he advocated its use that it was first brought prominently before the profession and extensively practiced. His facts and inferences, as recorded in his "Medical Reports," are so often quoted or referred to that it would be quite out of place here to attempt to give even an outline of them. All we can attempt is merely to make a few observations on the three following points:

1. The rules which he followed in the administration of cold affusion.

2. The forms of fever in which he employed it and his success in this treatment.

3. The causes which have led to the general abandonment of this practice in the treatment of the majority of these forms of fever.

First, the rules or rather the restrictions which Dr. Eancer observed in the administration of cold affusion in fever were nearly universal:

1. When there is present a sense of chilliness.
2. When the heat of the surface is not steadily above what is natural - or

3. When there exists any general or profuse sensible perspiration -

We have stated that Dr. Currie was not the first modern physician to use cold affusion in fever, but so far as we have been able to ascertain, he was the first to announce their restrictions, under which, and under which alone, it can be employed with efficacy or even safety. Many illustrations of their importance occur in the course of the Reports. As a specimen we may take the following:

In Vol II, p. 224, Dr. Currie states that between the years 1793-97 several American physicians employed cold affusion in intermittent and yellow fevers, with occasionally very marked benefit to the patient, but in other cases that not infrequently with the effect of causing, or at least hastening, a fatal issue; and this of course soon led to the abandonment of the practice entirely. The secret of their want of success was their ignorance of Dr. Currie's restrictions. Their leading idea seems to have been that the affusion of cold water acted simply as a stimulant and tonic, and necessarily to useful where stimulant and tonics - both and tonic - are indicated. This affords a striking illustration of once of the unsatisfactory state of therapeutic nomenclature, and of the serious errors into which that nomenclature often blindly followed is apt to lead - leading as it seems to have done in this case, to the administration of affusion.
in the cold stage or during the period of post-febrile disability, with the very indifferent success we have just indicated.

But we hasten to notice the forms of fever in which Dr. L. employed cold affusion. There were various of these principal being, typhus, scarletina, and intermittent, which he used it also with very marked benefit in the form attending influenzæ and smallpox. Of the two latter we cannot now speak but of typhus. This is the only form of fever treated by Dr. L. which there can be any difficulty in identifying. From the descriptions however which he gives of the fever there can be little doubt that the majority of the cases he treated by cold affusion were cases of true typhus, corresponding as they did with remarkable uniformity to bullæus well-known definition of that disease. The diagnosis of the true typhus from the relapsing and from typhoid, had not then been agitated; indeed it may be doubted whether the relapsing form then existed. There do appear however indications which would lead us to the conclusion, even although the characteristic cutaneous and intestinal affections were not observed, that he did meet with cases and even epidemias resembling typhoid. Thus he speaking typhus being not very infrequently "complicated with diarrhoea" and this he believed to "forbid the use of cold affusion or at least to render its advantage uncertain." Now this fact taken in connection with what
he states in the following quotation does, in my view, favour the conclusion we have just indicated. In his paper he says: "The typhus at that time was during the Autumn of 1831 and while a very fatal form of dysentery was epidemic in Liverpool, a fact it may be, not without significance in reference to the point we are now considering. The typhus at that time differed somewhat from its usual character among us. It came on in a less obvious manner. For some days the patient felt dull headache, languor and debility, with slight chills at intervals and uneasy night. The tongue was slightly furred. In its regular course the disease was prolonged beyond the usual period of typhus, extending to eight or twenty days or even more, and sometimes leaving behind it for a week or two a state of mental torpor and physical exhaustion. The skin was at no point after the first or second day, particularly dry or contracted; and in twenty three cases which I attended closely and examined with care I did not find in a single one the heat exceed 102° at any period of the disease. In general the heat was from 98° to 101° and greatest about the fifth or sixth day. Of the twenty three patients which I have mentioned I lost two. Both of these had undergone extraordinary fatigue." 

"In the treatment of the other twenty one cases there was little difficulty. The affection seldom lasted more than four days generally tepid was employed in all of them, sometimes once and more frequently twice a day."
"It was always followed by refreshment and immediate relief, but had less effect in cutting short the disease than usual - I did not even succeed in stopping it in cases in which the remedy was applied on the second or third day; but this might be owing to my seldom having recourse to the perfectly cold affusion from the unusual deficiency of heat in this epidemic. Perhaps a practice somewhat bolder might have been more successful."

We have given in full this somewhat lengthy quotation both because it details symptoms not at all unlike those of typhoid, and thus confirms the conclusion that the form of fever in which cold affusion was usually employed was the true typhoid, and also because it contains Dr. Currie's account of the only epidemic in which in the course of ten years during which epidemics of fever were very frequent, in which he found that the cold affusion even when used early in the course of the disease did not exhibit its wonted power of cutting short the malady. This reason of this failure we think he himself indicates when he states the simple fact that it was so seldom tried. We cannot speak with any authority on the point, never having tried the remedy; still we conceive that whilst Dr. Currie here displays a laudable degree of caution in the application of a comparatively new remedy, he might have been more successful if as he expresses it he had followed a bolder practice, or rather, if his caution had been marked by greater skill.
The comparatively low degree of heat present did not in itself indicate that it was necessary to apply the affusion at a temperature higher than ordinary, but merely that the duration of the process be shortened, and water be passed from a greater height, or in other words that something of the tonic action of the affusion be secured.

The mistake into which Dr. Gurney here falls seems to have been that of supposing that the therapeutic action of cold affusion depends merely on the quantity of heat abstracted, and consequently that when there was not a large quantity to be abstracted, its administration could not be expected to be beneficial or even safe. Now we believe that here as in the case of the abstraction of blood curative action depends less on the more quantity abstracted than on the manner in which the abstraction is effected ... Here it merely quantity on which efficacy depended, according to Dr. Gurney, even showing, "in many cases the heat of the living body is lowered as speedily by the affusion of tepid water as by the affusion of water that is cold," if I mistake not, "it holds in some cases the heat is lowered more speedily by the tepid water." Vol. I. p. 69. Dr. Gurney, therefore, of the experience of 1801 we have the anomaly of employing the most powerful means of abstracting heat in a form in which no leading feature was a low degree of morbida heat. This subject will again come before us in the examination of the rationale of cold affusion.
We therefore leave it for the present and go on to notice Dr. Barrow's alleged success in the treatment of the true typhus of his day—his own statement on the subject is that "in the first stage of this fever the application of cold water appears very generally to cut short the disease almost instantaneously; and even when it fails of this effect, as is usually the case when it is applied in the more advanced stages, it nevertheless moderates the violence of the symptoms and shortens the duration of the disease.

This is surely claiming a good deal for cold affusion, almost all indeed that could be wished or at least expected in any remedy, yet we believe the statement to be fully borne out facts occurring both in Dr. Barrow's experience and in that of others recorded by him.

Many however have doubted this. Thus Dr. Watson in speaking of emetics and cold affusion as expected means for cutting short fever says: "In the few instances in which one or other of these remedies may have seemed to arrest the fever or to check its progress that effect has always occurred at the very commencement of the complaint; so that we cannot be sure (and the probability lies the other way) that there were really cases of fever at all, or that they would not have ceased even if nothing had been done for them." Lectures on Practice of Physic Vol III. p. 767

A considerable period must surely have elapsed between the time Dr. Watson read the "Medical Reports" and the time he penned this passage, otherwise he could hardly have forgotten.
so completely as he seems to have done. It becomes, facts, or have alleged that there are only a few instances in which the cold affusion has seemed to arrest the fever. It is impossible to give Dr. Burris' actual number, but certainly his statements leave the impression that it is not 'a few' but in hundreds of cases, the affusion did 'tend to arrest the fever.' Whether the affusion ever more than arresting is a different question. But Dr. Burris and the numerous other practitioners, some of them the first men of their day, who followed his plan of treatment, must surely be regarded as being capable of telling us what intend to take place, and their unanimous testimony is that in the form of typhus prevalent about the end of last century and the beginning of this, the affusion of cold water did. come in a very large proportion of cases, when applied early in the disease to cut it short. In fact it 'seemed' to do to such an extent that they all came to the conclusion that it actually did so.

We have already stated our conviction that their experience warranted this conclusion. Now, this conviction shaken by the objection urged by Dr. Watson, on the ground that the affusion being employed very early in the disease it cannot be proved that they really were cases of fever at all, and would have got well as quickly left to themselves. No doubt cases such as he supposes do occur, but we have no evidence that during epidemics of typhus, they occur in anything like the proportion in which the affusion was employed with success. And besides this, there are numerous cases related in which the symptoms of fever were quite unmistakable on the third or fourth day of the disease, and in which nevertheless
it was arrested by the cold affusion.

In answering the question whether this is any mode of arresting from immediately on its formation Dr. P. Williams in his "Elements of Medicine," Vol. I., says of cold affusion as one of the means employed for this purpose. "A fever circulating with the blood cannot be removed from the system by ablation of its surface. No person expects to stay the course of small-pox or scarlet or of typhus by a similar application. We might therefore have predicted a priori that cold affusion could not remove from the body the poison of typhus fever and consequently had no power to stop the course of the disease though it might modify the symptoms."

This requires no answer. It is simply a petitio principii - a species of logic which has in therapeutics, as in every other department of human knowledge, done more harm than perhaps anything else to retard the discovery of truth.

Such being our belief in the efficacy of Cold affusion in the typhus of the beginning of the century we have now to inquire into the causes which have led to the general abandonment of the practice. Here it is obvious are to be sought either in a change of type in the disease or chiefly in a change of fashion in its treatment.

Both causes have been at work, the former to some extent at least. The cause of the latter - Absolute certainty on this point however is impossible without acquaintance with the history of Cold affusion from Dr. Cursiv's day down to the present, than we have been able to acquire; but that such have been the causes of
its abandonment is, I think, rendered highly probable by what is stated by Dr. Christian in his Article on Fever in the Library of Medicine Vol. 7, page 173. He more states, that in the first great epidemic of typhus - that of 1817-20 - after the promulgation of Dr. Cullen's plan, cold affusion was used by many practitioners with the greatest care and perseverance, but even when applied early in the disease, it very rarely had the effect of cutting it short, though it did temporarily relieve some of the symptoms. This comparative failure was as Dr. Christian observes the more remarkable from the circumstances that that epidemic "owed its eminently inflammatory character and the high power of reaction of its early stage to be exactly the form of fever for treatment by cold affusion." The prevailing type of fever in the epidemic of 1817-20 was that known then and subsequently as the "Relapsing typhus," a type which has repeatedly characterized later epidemics. Now by its want of success in arresting the progress of this type of fever during the first epidemic, the cold affusion lost the prestige it had gained, and accordingly in subsequent epidemics, whether they were precisely of the same type or not, all hope of cutting short the disease by its employment being gone, other less heroic application of water were found to palliate the symptoms and as palliatives have been generally adopted - cold affusion has thus to a great extent been superseded by tepid affusion, cold or tepid drenching to - it changed then in the type of fever and a consequent change in its treatment.
accounts so far at least for the disappearance of cold affusion from the list of the febrifuge remedies, appropriate in typhus.

Perhaps another cause of its disappearance from that list is its power for evil when carelessly administered for that when thus administered it may cause, in fact, has caused death. There cannot be a doubt. Proof of this we find in what has more than once occurred in the Charité Hospital of Berlin; where Dr. Horn in the early part of the present century employed affusion in an unusually fatal epidemic of typhus, with signal success, and where something of the same method of treatment has even since been practiced. There according to Dr. Armitage, about seven years ago, the administration of the baths was left very much to the discretion of the bath servants and nurses “so that” he says “at the time when I arrived at Berlin the method had so degenerated that it could scarcely be recognized. A very powerful douche had been substituted for the much more troublesome practice of affusion. I have frequently seen this allowed to fall for several minutes together on the head of the unfortunate patient. It is no wonder then that such a practice should frequently be unsuccessful, and that the whole method should fall into disrepute. I have been told by the bath master that he has seen several cases of death while the patient was actually under the douche. If such abuses were the necessary accompany
of the practice of cold affusion, we could not but con-
gratulate ourselves on its abandonment in this country; and
that these or similar abuses are very apt to
creep in we may infer from the fact that they were
then going on in the Charities of Berlin, which it was
under the care of such men as Schönlein, Wolf,
and Treuthe. The prevention of such abuses
however requires only that the attention of the physicians
be directed to the possibility of their occurrence, and the
being done, we cannot but feel some regret at seeing
a method of treating typhus which unquestionably
was at one time highly successful, disappear to such
an extent as it has done in Great Britain. For it
is more than possible that not only in individual cases,
but in whole epidemics it may again with advantage
be employed. But it is obvious that unless it be
tried now and again, in cases where it is certain that
it can at least do no harm, epidemics after epidemics
of typhus of that very type in which cold affusion
might cut short the disease may be allowed to pass,
without its being ascertained that such really was
their nature.

Having thus spoken of typhus, cold affusion in typhus,
we now proceed to glance at its application to another
form of fever—
2. Scarletina. The variety of the disease in which Dr. Burnet
seems chiefly to have employed affusion was the Scarletina
anginosa. The patient heat which characterizes the early
stages both of this variety and of *S. Malagine*, seems to point them out as peculiarly suitable for such treatment.

This very intensity however, increasing as it does the importance of the abstraction of heat, increases also the importance of so managing the process of abstraction as to prevent as much as possible the redevelopment of the heat. This is to be attempted by seeing that the temperature of the water employed be not very low, that the affusion be continued for at least a few minutes on each occasion, and that the water reach the patient from a very slight elevation. These cautions were not observed by Dr. Burrie, for we find him stating that his plan was, "to strip the patient, and draw four or five gallons of the coldest water to be poured over his naked body. This produces its usual cooling effects; but these are less permanent than in typhus. In one or two hours afterwards the heat is often found on examination as great as before. The affusion is therefore repeated again and again as the obstinacy of the heat may indicate. It is sometimes necessary to use it ten or twelve times in twenty-four hours. At the end of this time, but commonly earlier the force of the fever is broken and a few tepid affusions at longer intervals are sufficient to subdue it entirely."

Now it may be said and said truly, that no merely theoretical considerations are competent to determine the question whether the kind of treatment we suppose above or that here recommended is really the more suitable
Such considerations, however, are competent to warrant the doubt whether Dr. 65's preference in these circumstances, for "declining four or five gallons of the coldest water to be procured over the patient's naked body" was well founded; for it may be questioned whether he ever gave the cool or tepid affusion a fair trial. If in employing them he merely dashed a few gallons of water either cool or tepid over the naked body of the patient, their effect must have been very similar to that of cold affusion, only less decidedly cooling and by so much less efficacious. If however he had employed them as we have already indicated, their effects might have been found as indeed some practitioners believe they have found them—more decidedly cooling, and more efficacious than those of cold affusion.

On this point however we must remain in doubt till experience our own or that of others determines which is the preferable treatment. Meanwhile we may listen to Dr. Burn's account of the success of cold affusion combined as it always was in the later stages of the disease with cool or tepid affusion. "In all the cases," he says, "which I have seen during this period—a period of about four years, amounting to upwards of a hundred and fifty—I have uniformly followed the practice which I have just described and with a degree of success so nearly invariable, that I cannot contemplate it without emotions of surprise as well as of satisfaction."

Now nearly invariable his success required to become in
order to excite his emotions of surprise and satisfaction he leaves his readers to guess; much must have depended on the degree of success which he was previously in the habit of enjoying. Trivial and unimportant as this circumstance seems we have here an illustration of that want of accurate statistics which detracts so much from the value of the "Medical Report." The statistical method of inquiry was then all but unknown or we would certainly have expected Dr. Carrie to tell us what the average mortality of typhus and scarletina under the ordinary treatment and under that suggested by himself actually was. Be this as it may we find that scarletina is the form of fever in which the practice of Dr. Carrie has been most successful, especially when employed in the early stages in the hands of more recent practitioners. For though some regard spongeing or the tepid bath as superior the cold affusion has found advocates amongst some of the best writers on the treatment of this disease. As a specimen we may quote the statements of Dr. Bateman on the subject. We are possessed by a sense of physical agent so far as my experience has taught me not excepting even blood letting in inflammation, by which the functions of the animal economy are controlled with so much certainty safety and promptitude as the application of cold water to the skin under the augmented heat of scarletina. Favorable in the course of a few minutes the pulse has been diminished in frequency, the thirst has abated.
The tongue has become moist, a general free perspiration has broken forth, the skin has become soft and cool, and the eyes have brightened; and these indications of relief have been followed by calm and refreshing sleep.

These observations are quite supported by the experience of others who have tried the remedy extensively in Scythia. Indeed this is the only form of fever in which that cold affusion which Dr. Burrie advised, and that not without good evidence, to be so all but universally applicable in fever, has made a mode of treatment generally recognised by the medical profession.

We have now a word to say regarding cold affusion in only one other form of fever; viz. the Intermittent. Dr. Burrie's experience of affusion in this disease seems to have been limited. He saw enough however to convince him that it would form an efficient remedy. He says that in the hot stage he frequently employed it and "almost always with the effect of causing an immediate dissolution of the fit" and that in some instances "the succeeding paroxysm has been prevented by using the cold affusion about an hour previous to the period of its expected return, and the disease has been ultimately removed by continuing this practice through four or five of the following periods."

The gradual disappearance of intermittent{s} to do
great an extent in this country, as has taken place, have prevented Dr. Burnet's practice being tested as otherwise it probably would have been. So long however as the British Empire includes so many regions in which these diseases are endemic, any rational mode of treatment which promises to arrest or check their formidable nature, demands the attention of British practitioners. But while Dr. Burnet's experience has not induced many in this country, or so far as we can learn, in our colonies, to try cold affusion in aque, it has led its adoption by several continental physicians. Amongst others, by M. Fleury who about nine years ago gave the results of a memoir read before the French Academy of Sciences. This gentleman employed the remedy on two to four hours before the expected paroxysm, in the form of a general douche, and also in that of a local douche to the region of the spleen. He had eleven cases in all. Of these, seven were recent, the patient not having had more than from three to seventeen paroxysms; and having had no quinine. Of the seven, the plan was cut short in one case by a high application of the douche, in two cases by two, and in four cases by three applications. The other four had been ill from two to eleven months, had had several relapses, had resisted quinine and presented all the anaemia, lumbociation, and aorticitis which characterize the advanced stage of the disease.
Thus douches were required in two of these cases, and five in one other to remove the fever, while from eight to eleven were necessary to cause the splenic engorgement and the cachectic symptoms to disappear. In one of the cases the liver was greatly enlarged, but the condition also disappeared under the persevering employment of the affusion. Mr. Fleury's conclusions, which seem to be warranted by his facts, are "1st. That in the treatment of recent intermittent fevers, simple, and with little or no engorgement of the spleen, the cold douche may be substituted for quinine, and 2nd. That in the treatment of old standing agues, where several relapses have occurred, and there is considerable enlargement of the spleen or of the liver with a cachectic condition, cold affusions are to be preferred to quinine, for they cool the fever and restore the visceras to their natural volume, and remove the cachexia more rapidly and more safely than quinine: the latter in large doses does not unfrequently acting injuriously upon the nervous system or on the digestive organs."

We have thus noticed the three principal forms of fever in which cold affusion has been employed, and have, we think, seen enough to convince us, that when properly administered it really is a valuable remedy.

And now before we proceed to consider the other two methods and caluminous applications of water by the
warm bath and the cool sheet packing, we have a word today on the rationale of cold affusion. According to Dr. Currie the modus operandi of cold affusion in fever is the following: "The sudden general and powerful stimulus given to the system dissolves the spasm on the extreme vessels of the surface, and of the various cavities of the body; the sudden and general evaporation carries off a large portion of the morbid heat accumulated under the skin, and the healthy action of the capillaries and exhalations being restored the remaining superficial heat passes off by sensible and insensible perspiration. The stimulus of morbid heat and morbid structure being removed, the morbid association seems also broken by the sudden and powerful impression on the sensations; in fact the inordinate action of the heart and arteries subside, and the harassed and toil worn patient sinks into that peaceful sleep which nature has provided - the solace of our pains and sorrows and the restorer of our strength."

Such was the old theory which the pathology of the day - that of Hoffmann and Crocker admitted, and undoubtedly it contains much that is true respecting the action in question. The more recent views of fever, however, which seem to be founded on more correct observation of the phenomena, afford an explanation still more distinctly favourable to cold affusion as a remedial agent in that affection.
This we think will appear as we proceed. It would be quite out of place here to be entering at any length into the pathology of fever. We shall content ourselves with noticing a few of those points in that pathology, which seem to have a special bearing on the therapeutic action under consideration.

Whatever theory may be held regarding the priority of the affection of the solids or of the fluids, it is now on all hands agreed, that some lesion of the function of the nervous system must precede the development of the characteristic symptoms of fever. The supposed cause of this lesion we shall notice presently, meanwhile its results are derangement of the digestive, the respiratory, the circulatory, and the excretory systems. Of the derangement of the respiratory and digestive systems we shall not now speak, but of the other two known calls for consideration.

In regard to the circulation, it is now held that the state of the ultimate vessels in fever, is not as was supposed by Hoffmann and Ebell, one of spasmodic contraction, but one of excessive relaxation; and that it is in order to overcome the increased tenuity of the blood to be retarded in those relaxed vessels that the heart is called into more frequent and more rigorous action.

Were this the only morbid condition present there is one therapeutic indication, the securing of which we might expect to be almost specific in fever; that indication,
would plainly be, to cause contraction of the dilated capillaries: we would thus at once restore the circulatory system to its normal condition. But unfortunately for such a supposition there cannot be defined to be a dilated state of the capillaries. Nevertheless a remedy capable of removing that morbid condition would certainly be very likely to prove beneficial in a high degree. What remedy then may be expected to gain this indication? The ordinary physical principle that the abstraction of heat from any body causes contraction of that body at once suggests itself, and as the media for this abstraction both air and water may be employed: the former to the cutaneous and respiratory surfaces, the latter to the cutaneous surface and to the alimentary canal. Nor have such remedial means been unnoticed. They constitute what has been so long known as the "cool" method of treating fever, and its vast superiority over the "hot" method which preceded it, strongly corroborating that theory of the state of the capillaries in fever which now prevails.

It is known with only one item in the "cool method" that we have now to do viz., the abstraction of heat from the cutaneous surface by water as a medium and that medium employed as in cold affusion. As far as the capillaries at or near the surface are concerned, this application must certainly gain the indication of which we are speaking. But certainly the abstraction of heat by means of cold affusion
cannot be continued for hours or days, what good can be gained by the little more than momentary contraction of the capillaries of the skin, which it produces? We are not prepared to say that even this could do good, but certainly were this its only action, its therapeutic value would be small indeed. In order however to ascertain its real value we must go back to the cause of this relaxed state of the capillaries, this lesion of nervous function. Let us take then a case of fever in which this lesion is well marked—a case of typhus—where we have not only the flushed face and the accelerated circulation, but also decided cerebral sympotms, and then let us inquire how these symptoms are affected by cold affusion. Such an inquiry is easily answered; for no fact in the history of cold affusion has been more thoroughly ascertained than that the head symptoms of typhus are all but invariably relieved by it. And if we thus find that cold affusion has the effect of restoring function to those portions of the nervous system which furnish the conditions necessary for thought, sensation and voluntary motion, we establish a very strong probability indeed in favor of the supposition that it has also the power of restoring function to that part of the nervous system which furnishes these conditions necessary for the regulation of capillary circulation. If this it really possesses this power it would appear that cold affusion not only causes a momentary contraction
of the capillaries, but by the sudden abstraction of a large quantity of heat a shock is given to the nervous system which forces it to resume its function, and keep the capillaries not as much contracted as they are immediately after the affusion, still in that state of "medium tension" which is essential to the healthy discharge of their function. And since this is true not merely of the cutaneous capillaries, but of the whole capillary system, that which rendered necessary the increased action of the heart is removed, and hence there is a very marked diminution in the force and frequency of the pulse. Of seven cases detailed by Dr. Barrie we find that the average fall in the pulse two minutes after each affusion was 13.5 beats per minute.

But however favourable these results of cold affusion may seem, it is found that after some hours, it may be only a very few, have elapsed, the pulse again rises and all the other symptoms seem much as they were before the administration of the cold affusion, requiring the repetition of the remedy again and again, it may be, before any permanent effect is produced. Why is this? It is obvious that the real cause of the morbid phenomenon had not been removed, and in order to discover this cause we must return once more to that lesion of nervous function on which the fibril symptoms depend, and inquire into its cause.

It is quite conceivable that this lesion may have been produced by a cause now no longer in operation, but
may itself be of such a nature as to render it impossible for the nervous system immediately on the withdrawal of the noxious agent to resume its function. It is, however, we think, for reasons which will appear as we proceed, more probable that the continuance of the nervous lesion depends upon upon the continued presence of what at first caused it, or (according to the commonly received theory) some specific from poison.

Assuming then the presence as the system of some such poison, the paramount indication obviously is to have it eliminated as speedily as possible. A serious obstacle to this however presents itself in the fact that one of the first effects of these poisons is, as we have already indicated, to cause a lesion of nervous function such as greatly damages the organs of excretion whose special function it is to eliminate effects and morbied matters from the system. The consequence of this is the retention not only of the fore poison, but also of those wound elements of the tissues whose presence is scarcely less injurious. It is fortunate that the organs of excretion are never all equally thus involved, and that they have so much in common that they can and do act to a large extent mutually. In each of the different forms of fever, and more or less in each different epideme of the same fever, there is usually some one organ of excretion more affected
than any of the rest, thus giving rise to the idea that each individual from poison, not only acts on the nervous centres and through these disorders more or less the whole function of excretion, but is determined to some individual organ of excretion, in particular, on which its action is specially marked. Now the organ to which more frequently than to any other, this determination takes place is the skin. This is very apparent in the erythematosa, but is hardly less strikingly manifested during the hot stage of all fevers, in the hot and dry skin, so characteristic of the febrile condition. If in these circumstances the cutaneous excretion is not absolutely arrested, it certainly is very much interfered with; and this notwithstanding the fact that transpiration is far as that is the merely physical function of the skin as a porous organic membrane is owing to the elevated temperature considerably increased. The proper excretions of the skin, the sebaceous and the sudoriparous, are both to a great extent in abeyance. Consequently so far as they are concerned, the elimination either of a poison or of that effete matter which it is their proper function to eliminate, must be very imperfectly, if at all, performed. Hence the restoration of the cutaneous excretion must always in such cases be an indication of the utmost importance. How thin close cold affusion when repeatedly ad-
Meet this indication? As we have already stated it is
obvious that a single cold affusion cut short a
fever, and this seems to arise not so much from
its failing to gain the indication suggested by the
state of the circulation, as by its want of success in
securing the equally important indication pointed out
by the condition of the organs of excretion in general
and of the brain in particular. In order then to
account for the undoubted fact that the repeated
application of this remedy does succeed in arresting
the disease we must look chiefly to its action in
restoring cutaneous excretion. And here we seem to
be warranted by facts to suppose that the
shock of a single affusion acting as a tonic on the
nervous system, places it for a short time in
condition in which it is incapable of being affected
by the fever poison: that this interval is taken
advantage of by the nervous system for the purpose of
not only so far resuming its various functions, and
among the rest that of regulating excretion, but
also so far remedying the disease condition whatever
that may have been on which the derangement of
its function depended, and accordingly when the
morbid influence does once more make itself felt
its poison is found to be sensibly diminished.
If then this really is what occurs on each application
of the cold affusion, it is obvious that its repeated applica-
tion may be expected to have the effect of allowing the
morbid condition of the nervous system to pass off, and of restoring the function of execution.

We have thus noticed some of the modes in which cold affections may be supposed to act in restoring the functions of the circulatory and excitory systems; but we have as yet said nothing of its action in connexion with that disorder of function which of all others is the most marked in fever, and from which indeed the pyrexia derive their name—namely, the disorder of the function of calorification. Our reason for deferring its consideration till now is that while it may be common with the two preceding disorders of function, have some immediate connexion with that nervous lesion of which we have spoken above, it seems to be to a great extent, dependent upon these disorders. This is apparent when we look at the relation in which these three functions normally stand to each other; for the morbidly high temperature of fever seems to be caused merely by increased activity in the natural processes of calorification. That process then consisting as it does chiefly of the slow combustion of the hydrocarbons of the tissues by oxygen brought to them by the capillaries, it is evident that a dilated state of these vessels, together with the consequent acceleration of the action of the heart by causing a larger quantity than usual of oxygenated blood to pass through a part in a given time, furnishes the very conditions necessary for an increased production of animal heat. In all this, however, there may be nothing inconsistent.
with health, for in that condition the unusually large quantity of heat thus generated is not retained in the system, but combining with the unusually large quantity of moisture furnished by the augmented activity of the organs of excretion, especially that of the skin, is given off in the form of perspiration. In fact, however, we have seen that so far from being increased all the excretions, but especially the cutaneous are liable to be greatly diminished, the consequence of which must plainly be a morbid accumulation of that in the system.

Such then being the connection which subsists between morbid heat and deranged circulatory and excretory functions, it is obvious that if cold affusion has the effect of rectifying the latter it must restore the former. And hence we are prepared to expect what Dr. Cramer observed viz. that the affusion had always the effect of reducing the temperature — the average fall in the seven cases we have referred to being 3.25° Fahr.

We have thus considered what may be the supposed to be the modus operandi of cold affusion in checking fever, and have seen that its efficacy as a sedative depends primarily on its action as a stimulus of the nervous system. This clearly implies that in those cases in which is useful the nervous system is, pro tanto, in a state of impaired functional activity, and that the fact that it is useful in such cases would with equally clearness indicate
That in cases of an opposite nature, where there is excessive nervous irritability it would probably be found to be useful. Experience confirms the supposition. Thus Dr. Armitage speaking of affusion in this class of cases says "Whenever I have applied it in such cases I have repeated its use from the increased irritability, restlessness and loss of sleep caused by it."

Cold affusion being thus contra-indicated, there is another mode in which water employed externally as a medium for the abstraction of heat is used in the treatment of such cases. We refer to the Warm Bath. By this we understand a bath in which the patient is immersed up to the chin or at any temperature between 92° and 98°. We intended to have spoken of the well known calming action of this bath, but time forbids. We would only observe here we pass on that in order to secure the calming effect we must see that on no account the temperature be allowed to be higher than 98° and that the immersion take place not for a five or ten minutes, during which there is usually a little gentle excitement produced, but from twenty minutes to one hour over an hour.

We now come to notice very briefly the last three forms above mentioned in which water is employed externally and in which its action is modified by caloric viz. the Wet Sheet Packing. We shall not enter at all into its unknown history farther than to remark that although
we could perhaps have wished for it a more dignified origin, when we look into the history of some remedial
measures which stood high in the Pharmacopoeia, we discover much more of the secret of quackery, in the secrecy and
ignorant pretension which characterized this introduction into use, than we do in the adoption of the water dressing
and the poultice of the surgeon into the armamentarium of the physician — for such essentially seems to have been
the idea of the Selian peasant who first employed it.

The physiological action of the wet which has not so far as we are aware been accurately observed. We do,
indeed, find in "Researches into the Effects of Cold Water on the Human Body," by a Dr. Howard Johnson, what professes
to be an exact account of its action on the pulse and the respiration in health. That this is what it professes
to be we are very much disposed to doubt; for it appears that he took as the normal standard the pulse of a
man immediately after gentle exercise, and which he was still in the erect posture. Indeed the author seems
to thoroughly anxious to make out a good case for the wet sheet that he would have been very careful to have
told us had he really observed the precaution which any
man with an ordinary medical education seeking the truth
and not the glorification of a special method of cure
would have observed, namely, of ascertaining exactly
the state of the pulse and respiration in the subject of the
experiment lying in the position in which he was to be
during the development. So assured did we feel of this
that we took it for granted till we came to the following passage: "Next then in the time that it takes for a man to lie down and have the two ends of a sheet applied on his body, in one minute, sometimes in less than one minute is the pulse fallen nearly twenty two beats in the minute." Then by way of being emphatic he italics he repeats, "In less than one minute twenty two beats! It was not this before the discovery of the wet sheet in conceivable."

In order to test the probable accuracy of this very triumphant looking conclusion a fellow-student and myself made some observations on the effects which gentle exercise and position have upon the pulse. With the following result: After walking gently across the room once or twice we found that the average of our pulses was 91.3, while after lying in the horizontal position for one minute the average fell to 63.4, a fall of 27.7 beats per minute. Position alone then, reducing the pulse more by five beats per minute than the wet sheet. We do not give this above as, even an approximation to what may be found to be the average effect of position on the pulse, but to us it seems quite sufficient to invalidate the testimony of Dr. Howard Johnson's researches. The blunder which he commits, whether intentionally or unintentionally, convicts him of an amount either of knavery or of ignorance which renders his observation utterly worthless except as warnings in determining the physiological action of the wet sheet; and both recorded evidence being wanting.
we leave its physiological and proceed to notice its therapeutic action.

Here then is no lack of strong assertions in favour of
the wet sheet, but as they are intended chiefly for the
popular ear they are not supported by the kind of
evidence which a medical inquirer seeks and is
entitled to expect. We cannot but think, however, that the
testimony of so many observers, both in and out of the profession,
though they do not give their testimony in the most satisfactory form,
fully warrants the conclusion that it does act as a stimulant of
the heat of action and as a calmer of nervous irritability;
and not only so, but that according as either of these
actions is that chiefly indicated, the former action may
be varied by attending to the extent of surface over which the ap-
planation is made— the temperature — the degree of moisture—
the amount of covering over it and the duration of the
envelopment.

Thus when its stimulant action is that chiefly indicated, as in the hot
steps of fever, in which stage alone it can with safety be employed,
in form, the sheet is usually bared slightly, being cut out of cold water
made to envelop the whole body of the patient — is thoroughly
thrown not heavily covered with blankets, and is continued
till it is completely warmed but not longer. The
period will then vary inversely with the amount of
heat developed. When one sheet has been warmed, if
the fever is not completely abated, as in all probability it
will not unless the application has been made at its very
commencement, the patient is enveloped in a second, then
in as thick, and so on till the pulse becomes soft and falls in rapidity. As the amount of heat abstracted in any given time by each successive sheet is smaller the duration of the operation gradually lengthens. When it is judged that a sufficient amount of heat has been abstracted and the sedative action as indicated by the pulse, obtained the patient is rapidly subjected to a cold, cool, or tepid bath, and after being thoroughly dried goes off to bed, to be covered between dry sheets or what is perhaps better warm blankets. Then probably he will sleep soundly, perspire freely, and after some hours awake to find that the fever has entirely left him.

Such is the usual method of employing the wet sheet in fever and such is said by those who have tried it to be its usual result. No case however of which we have been able to discover the details, and which would seem to us to warrant the conclusion are those treatment is a somewhat different method by Dr. H. Stallard in the Leeds, Temporary Fever Institution, and recorded by him in the British and Foreign Medical Review for January 1847, p. 267. His plan of treatment goes this: "The patient was stripped naked, enveloped in a cold wet sheet, and covered with a blanket. After remaining in the situation from 10 to 15 minutes he was instantly being dried immediately wrapped in a blanket thoroughly heated before the fire and then removed to another bed and covered over with bed clothes. He cannot have give the details of his cases, but in fact, no one can read them without admiring that they justify his statements regarding the effect of the wet sheet. The effect produced by the wet sheet is, first, a sensation of great cold.
accompanied with slumbering, but this is almost immediately succeeded by an agreeable sensation of coolness and comfort: the sheet then begins to grow warm and when the heat of the skin has been previously very great the blanket cools with steam. Shortly after the patient is removed to the warm bed, he begins to perspire, his headache and muscular pains cease, and he sinks into a calm and undisturbed sleep from which he awakes still perspiring, but painless, refreshed, and occasionally well.

We bring forward the above as evidence of the salutary action of the wet sheet, but it is obvious that the caloric element also enters largely into its action. When however it is employed in a case in which circulation is but slightly disturbed while nerves irritability is excessive and the heat of the surface not so much as it usually is in such the sheet is well thrown out of cold, cool, or tepid water, and made to envelop the whole or only a part of the body, according to the amount of action desired, it is then covered with a considerable quantity of blankets and the envelopment is continued for from 20' to 60' after the sheet has become sensibly warm. The effect of this is most soothing in the extremity, but since we have been unable to find any properly recorded cases in proof of this action, we must draw this very imperfect sketch to a close with a few words on the mode of operation of the wet sheet. Before doing so however we might have spoken of a modification of the wet sheet, the blanket being out of hot water, but, though this is a mode of employing water therapeutically, which is now coming into general use, and which will be better to found very serviceable, time and space both forbid.
The mode of operating of the wet sheet as a sedative must
to a great extent be identical with that of cold affusion.
In both there is the sudden abstraction of a large amount
of heat, in both consequently, there is that sudden shock
to the nervous system on which we have already seen so
much of the power which cold affusion possesses, of
cutting short fever depends. Beyond this point however
this operation must be widely different. After the affusion
we have reaction taking place immediately, and while
this is quite distinct from febrile exacerbation it certainly
lessens much more favorably to its occurrence than
is the thoroughly graduated reaction which we find
in the wet sheet. Nor is this the only advantage possessed
by the wet sheet; it employs the heat which on its
application it abstracts from the body, to convert into
vapour the moisture in the sheet so that as has
been observed "the patient may be said to lie in an
taporous bath of his own making." It is, in virtue of
this mild vaporous bath which forms part of the process
that the wet sheet possesses that directly calorific
action which moist warmth is so well known to
exert on the nervous system.

We regret that we have been unable to do anything like
justice to the subject of the paper. The great drawback we have
all along felt to be the want of accurately recorded facts. We trust
that in the future the want will by cautious experiment and careful
observation be supplied, and that wet, as a therapeutic agent will have
been assigned to it its proper place in the Materic Medicine.