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Thesis
for the
Degree
of
Doctor of Medicine,
by
Herbert Taylor.

University of Edinburgh.
March 1864.
Remarks,
on the
Practical and Clinical value of
Thermometric Observation
in disease, with facts, relative to the variations of the
Temperature of the body, in some forms of Specific Fevers.
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In the following Pages, I have endeavored to avoid
any attempt at theorising, more especially, as I am convinced
that theories can only be arrived at, as the result of long
continued experience, most accurate and unbiassed
observations, combined with that accumulation of
knowledge, which age alone can supply, but, which is
so essential to the forming of ideas on a right basis,
as well as to the prevention of too hastily formed
conclusions, which, in the almost invariable error
that the ardor of a less matured age, is so likely to
lead the young theorist into. Not that I think the
student or graduate, should defer a formation of
association of his ideas, till age has given weight
to his statements, but, let him, rather from little
theories for himself, husbanding them till they are
either overthrown by convictions, or confirmed by future
investigations, content himself with an accurate
and unfallacious notice of facts, which, if accurate they be,
will prove of essential service, when he feels himself
competent to offer his views to the criticism of others. Indeed, with these ideas then, I have abandoned all attempts at theory, and contented myself with simply noting facts, which, if they only confirm the observations of various writers on the subject of this essay, will not, I hope on that account be the less acceptable, or deemed superfluous. For, nothing tends more towards strengthening a theory than a constant confirmation or corroboration of the facts which are put forth to explain it. But, on the other hand, they differ from any statement already made. I humbly offer the pleasing thought: my present observations will perhaps be the more useful, if, that they might perhaps continue to a more searching and prolonged investigation, in order to settle the point of issue.

If I may be pardoned for sounding "Eps," I would say, in justice to myself, for the purpose of giving some little weight to any observations I may have made, (especially if differing from some of others!) that I have used the greatest care. The "more than accurate," to speak in the presence of my investigations, carefully trying to avoid all fallacies, and according to only any observations that seemed to me to have an air of doubt or
suspicions, in regard of exactitude, from however slight a cause, such doubt may have arisen I am the more satisfied of the fidelity of what I offer, as I think that I have wasted the elucidation of my investigations considerately, notwithstanding the temptation not to test to obtain what has cost no small trouble to procure.

But, while I am thus advocating my results for their accuracy, I cannot unfortunately with confidence assert my eyes to the fact, that these observations, extending as they do over a rather brief period, preserve as a necessary result, a somewhat limited research as regards individual instances of disease. To remedy this as much as possible, I have endeavored to confine my observations to the febrile and gastro-enteric forms of typhus fever; with this view I have availed myself of the facility afforded me for research, in the other wards of the Infirmary besides the ones with which I have been more particularly connected. Other cases besides those of fever have been observed by me, with reference to any subject as they presented themselves. With this preamble then, I will commence my subject.
It seems to me, that, the use of the Thermometer, with regard to Clinical Value, Diagnostically - Prognostically, is not by any means sufficiently recognised or appreciated. For although Thermometric Observations in Disease are being more attended to at the present day, still, the known facts, especially by English Observers, are few, and require a much greater development. Than at present: indeed, one or the Profession will be content to regard the use of the Thermometer in Disease as a "thing" of value.

In confirmation of this remark, I might instance the following. That in the wards ofently any Hospital, whether surgical or Medical, over such an instrument as a Body Thermometer, still, there is the same opportunity that presents itself, in course of time, for investigating the relationship of the Heat of the Body in Disease generally, as importance tested, are completely thrown away. This is the more可惜, in that, many Physicians likewise Surgeons, if questioned are ready to admit that a certain value is to be attached to a timely knowledge of the elevation of the temperature of the body above a normal point, as being the generally the chief, and sometimes the only means of becoming aware of the existence of some impending or latent disease.
or of the relapse of a previously existing one, or of some complication at a period of convalescence, while at the same time they omit to put into practice, Clinical Thermometric Observation.

This is clearly a very insufficient recognition of the use and value of the Thermometer.

Again - some physicians, perhaps, place a somewhat greater reliance in its use, and directions to "take the Patient's Temperature." This is done, but too often indeed without the slightest regard to accuracy, fallacy, or influence. It is perhaps taken once a day only, to what at very slight and irregular hours, the report is made, and the Physician feels either satisfied with the result, or makes use of the fact, if it confirms any preconceived ideas of his own, or new the designation by disregarding it as of useless value, if it afford him not a basis for diagnosis or prognosis.

Here the use of the Thermometer is improperly appreciated.

Now why should Thermometric Observation be so of so much importance?

In the first place it often affords the chief, occasioning the sole, inconceivable the confirmatory means of diagnosis, if, not only, some disease, whether
including or latent, (but frequently indirect, its actual nature) but also of the balance of probability one, or of some complications setting in at a period of convalescence. This is beyond "Yea or Nay." There have been a case of Pneumonia following complete recovery from an attack of leucy, diagnosed at an impending febrile attack solely by a careful prolonged use of the Thermometer during convalescence without any other indication whatever if the invasion of any disease being present at the outset. But the elevation of Temperature alone, without acceleration of Pulse (of any moment) increase of Respiration - Pain - or any other symptoms, thereby an intercurrent attack of Pulmonary disease might be suspected, led to a careful examination of the Patient, and the physical signs of the earliest stage of Pneumonia were discovered, for some time before the disease assumed itself by a manifesting of the ordinary symptoms attending this disease. The case terminated fatally, the patient being permitted Pneumonia, having in some portion of the lung advanced to the incorrectly described state of Gray Deposition with Abscesses was found. Treating the truth of this statement it tends to prove that there not only in reality
but value to be placed on Thermometric Observation. I have likewise shown that it is in some cases the earliest
means of establishing a diagnosis. How valuable this must be in point of treatment—indeed, to a
considerable extent, exercising an influence on prognosis is self-evident. Although this is but a hint at
what my memory furnishes me with at present, still it is a genuine one, and one wholly worthy of credit,
acceptance, and use.
As a further confirmation of my statement
Vol. i. p. 44. 7. 4. — lb. p. 44. 7. 6. — lb. p. 46. 7. 10.
Of Diagnosis of Relapses in cases of genuine SYPHILIS
Fever (by which I mean the gastro-enteric class of Acute
Fever) of the development of Tuberculous Pulmonary, at
the close of this & other forms of Fevers—have seen many
instances. Then really the symptoms present (aroused
by Thermometric Observation) might easily have been
attributed to a late convalescence from the
original disease.
In the second place: Thermometric Observation is of
importance in a Prognostic point of view as an
Illustration of which I would mention the fact, that as far as has hitherto been observed, an elevation of temperature above 105° in cases of Rheumatic Fever has proved fatal. For an instance of the veracity of this statement, vide a case of Professor Rujo's recorded in Mr. Allen's book, Vol. ii p. 229.

This is most worthy of careful consideration on the part of physicians, as it is of the utmost importance, prognostically. Perhaps eventually as regards a criterion of our treatment in this disease especially, for we know that in many of the other forms of fever, such as the Septic, the classes of the Endemic group of Specific Fevers, whether cutaneous as Erysipelas, Gastro-enteric as Typhoid.

The Septic-Septics as Erysipelas. The so-called communicable classes, nearly allied to the Septic-Septics as Erysipelas, its varieties.

The Malicious or Septic classes of the Endemic group of Specific Fevers, as, Intermittent, Fiebre, etc., especially in the General class of the Septic group of General FEVER as, Pneumonia, Pleurisy.

I know it may seem that the three different forms of fever, their varieties may have an Elevation of
Temperature, above this fatal standard of heat, at the commencement of the disease without deviation returning, or rather of receiving any injurious effects, is considered an evil prognostic.

Again as affording the earliest means of determining the critical stage in the specific fever, the thermometer becomes of essential value, more especially in the jaundiced hectic form of erysipelas or typhoid fever. These we know so well that the constant variation of the pulse peculiar to this disease, furnish un this reliable data as to the supposition of crisis. We are all further aware that the prosecution of each critical stage is of vital consequence, and must materially influence our prognosis. In the typhoidal disease, perhaps more than this, is greater care required on the part of the physician in point of treatment, thereby rendering a means of correct prognosis the more to be desired.


It would be interesting also practically useful to investigate thoroughly the maximum elevation of temperature compatible with life, both in general and in specific fevers, assaying the further effect of maximum elevation in the respective classes of the respective groups of its former and the relation existing.
between them — as, likewise, in the different varieties of the Endemic Epidemic groups, of the latter. Further, whether any relation exists between each maximum rise in the Articular class of the Syphilitic group as, Rheumatic Fever, to one of the febrile groups of the Endemic Specific group as, Pyemia which I am inclined to think may be nearer than is obvious at first sight.

In the 2nd place it must be allowed, that the Thermometer becomes a valuable guide to us in regard of treatment. For it is set down and recommended by many authors, that in fever one of the 1st points is to "reduce excessive heat." Vide letter Vol. I. p. 247.

Hence, whether or not the drugs or remedies we have employed with equal views have had such effect, we must be ignorant of, until the Thermometer tells the actual state of things before us.

I trust that these arguments I have endeavored to set up in favor of the value, use of the Thermometer in disease will be recognized and allowed.

Now as to a means of establishing these facts, it is obvious that we should possess an accurate knowledge of the days and falls of temperature in health before we can obtain the full value of the variations.
The heat of the body is subjected to an increase - but in attempting to arrive at the former - a great difficulty obviously presents itself. Sprengel thinks that until he have a more positive basis to work on he must be content to accept the statement of Taube - Randerlich & others - that the normal temperature of the body in health is 98°F allowing a variation for external & other influences in the system of a single degree in either direction.

Cited: Lebrae. 1864. Vol II. P. 64. Tr. W.

By accepting then this mean, as the average of health the presence of disease is at once determined by observing any deviation from such standard exceeding a degree whether a rise or fall.

Now in order to arrive at results of the temperature of the body in different diseases, their correlation the greatest accuracy is requisite, to obtain which the temperature of the patient should be taken during many different hours in the course of each day - more especially when taken only 2 or 3 times daily - as near the same hours of the day as possible. I have known many instances in which the thermometer has only been taken once daily, & that at dissimilar hours, & the results so obtained supposed to convey
a fair idea of the average heat of the body during the
illness, whatever such may have been. Observing
this can lead but to error. Another point may be
mentioned, consisting in patients, constant, and especially
at a period of convalescence, having been exposed
immediately prior to or being at the time, usually
left apart for regular daily observation. It is no
uncommon thing indeed to see patients at such
times, sitting up in bed, washing themselves, taking
their meals, or even out of bed, sitting on the foot
of the bed. Such exposure cannot allow of correct observations being
made, if the patient must be allowed time to
regain his natural temperature by returning to bed
for some few minutes, before he be thermoregulated
so as to show how amusing progressive is the fever
and delay. Perhaps indeed there time is an object
in limiting clothing more truly to the patient's
thermometer itself should not be too thick
in the bulb, so that it may be adjusted as closely as
possible. As all kinds of illness, especially where
children or adults are concerned, prominence in the
investigation.

I may be graduated various from degrees to
I think the gradation should never be less than 10 degrees—but it seems to me that one of 10 degrees is the
In the better and generally sufficient for practical purposes, for although a division into 10° or 11° will furnish a still more accurate register, yet, by noticing the intergradation of the 11°, it can be taken as 10°, the difference between which and the 11° being comparatively insignificant for clinical purposes, having the advantage moreover of allowing the instrument to be of less length. Now as of a shorter duration.

The Thermometer should always be tested by a standard, as usually purchased instruments are frequently very incorrect, even to a difference of 2°, which must obviously lead to the most erroneous facts.

Now, as to what part of the body should the instrument be placed?

To a greater extent to be secured, or more advantage be obtained, from one part than another? There are 3 parts in which the Thermometer may be placed.  

2. The Mouth.  
3. The Axilla.  

As to the Rectum, it will be at once seen that this is almost in every case impracticable, and otherwise no advantage could possibly accrue from its exercise.
B. The mouth: This is a most inconvenient spot, being far more tender to the patient, unless of sufficient adjustment. This therefore reduces us to the remaining part—viz.

4. The axilla: This will always be found to be the most convenient part—being perfectly reliable, easy of adaptation, & simply sufficient for all purposes of clinical observation.

As to the method of applying the instrument:

Some peculiarity may think it quite superfluous to state this detail, as it is an apparently obvious & "prima facie" matter—but I am convinced that by a careless adjustment of the thermometer, so much fallacy will arise that I am induced to offer a few remarks on the—what I consider best mode of application.

The bulb of the thermometer should be placed well into the hollow of the axilla—the shoulder & arm of the same side dropped firm down to the chest; the forearm carried as far across the thorax as possible, the libell cloth tied on to under each shoulder. By this means the thermometer is held in tight & secure, without any effort on the part of the patient, & without giving the observer the trouble of holding it in—
In other words it is essential to see that the patient be well covered in -

C. As to the sinus, that the instrument should be left in situ!

The best guide as to whether any material has been reached, becomes more, as he, in every segment of the egg, the scale, at each investigation, we soon see there a scraping elevation has been reached. On an average 1 min. is an ample allowance, provided always that the thermometer be previously heated, and the mercury allowed to fall which it will do with a greater repetition than once.

The knowledge of the period of the disease is also a good guide. Let me explain my meaning better. If it continues to “thermograph” a Patient when Convalescence is established, there will be in all probability a rapid fall to a normal standard or lower rate. Providing the instrument be previously heated, and hence if such be the case, we need not lose much time in waiting to see if any fluctuation will take place. But it is as easy matter to describe this matter, & it is a method long required when common sense is used.
That the thermometer should be read off without removing from the body, is almost too obvious to need notice—Yet, as I have seen the contrary done, it concerns me that it is worth of remark.

By attending carefully to such simple precautions as these—thermometric observation may be carried on with valuable & reliable results.

I find little writing to the foregoing, that the path of the matter is noted inchildren—Vol. i. P. 35. Speig.

threatened more than the path being of my Demesne, coinciding so closely with his own, as to warrant the supposition, that I have endeavored to palm off his conclusions as originating with myself.

Char however as conclusive as assert the contrary that I have allowed my matter to remain in “blatant utter” t & these remarks are worthy a place in his volutus—they cannot be wholly irrelevant in the present Essay.

He should also endeavor as much as possible to acquaint ourselves thoroughly with the different influences that may be exerted by

1. Age.
2. Sex.
3. Atmosphere. Smoker says, as low Temp. reduces the heat—

4. Food & Diet. — ordinary sick leaves — infusions
living as beer, tonic, coffee, mince meat, etc.
vide: Ik. 122.

5. Drugs. — digitalis lowers the heart.
uretines, as quinine, 372.
vide: Ik. 124-25.

6. Remedies. — hot thick leaves but only temporarily.
It is salt. — vide: Ik. 120-21.

Thus the temperature is lower than in the opposite condition,
but this is at influence hardly pertinent to our object.
If we wish to fix a maximum point of elevation in different diseases, we cannot do so with great
accuracy, scep., with perhaps definite prognostic
value. to allow such influences by hypothesis.
that the temperature also varies at different
hours of the day is a matter that must of necessity be
borne in mind, in proceeding into these
your cause or causes of the production of heat in
elevation of temperature of the body — unfortunately this
information indeed is wanting — great differences of
opinion existing on this head, than which nothing
is perhaps more certain than that but little is known
of the matter. — I would think it out of place to
in a paper like this to dwell its content with all
The different theories on the question, which though perhaps not numerous are still difficult of collection, add to which an incompetence on my part to give any original ideas on the subject, which I do not hesitate to confess, when I find other heads than mine so totally disagreeing.

It would however be a matter of great interest indeed practical benefit if any indisputable theories of causation could be advanced whether such causations are alike in all diseases or whether in different classes of diseases different causes exist.

Having now spoken of the Clinical use and value of the Thermometer in disease, its method of application, I shall content myself with the probability of the existence of certain influences of the weather. Knowledge of the causes of production of abnormal heat. I would wish to say a word on the significance of fluctuations of temperature in disease, but altho' I have carefully studied Trendelenburg's remarks on the subject with care, 1860. Vol. II. 64-65. Still they are already translated in letter Vol. I. 82-85, hence it will be easier for me saying to refer the reader to that book than give the ideas themselves in this place.
I will now adduce to the diagrams or tabulated sheets appended at the end of this paper to fully understand which, a few explanatory words may be necessary.

The two forms of fever I have more particularly investigated are shown by different signs.

The one, indicating Typhus Fever, or (I. entericorum)

Typhoid, or (Gastro-Enteric Typhus)

without Eruption.

In each diagram, also separate columns are drawn representing for

1. Day of Month
2. Day of Bicesis
3. Hour of Day.

Again, one half of each sheet (that colored with

black ink) indicates the fluctuations of Temperature

The other half (blackened with red ink) shows the

variations of the Pulse.

In 2 of the sheets a red tracing will be observed in blue ink, which indicates the number of

Respirations.

The observations of each day are marked off by transverse lines rendering the daily

fluctuations at once apparent.
Out of 16 cases of which I thermometers, I find only 11 are available as far as accuracy is concerned. Various circumstances having arisen to render the observations of the Commander incomplete and unsatisfactory. It may be thought that the number of cases is small, when consideration is given to the number of admissions of those in this paper cases during the past few months, but a little reflection will secure conviction that my limited number of observations in this class of fever, was largely due to the fact that patient seek for admission at different advanced periods of the disease, and many are admitted or very commonly the 8th frequently a later day. Which affords comparing little interest thermometrically regarded, in a disease usually running the brief course that does the more so as we lose the observation during the first week, which is of prime importance. The involution of the form, being slower, and centering the loss of a few of the premorbid steps of less significance, instanced as the development is later, the course prolonged.

For further explanation of the limited number of cases, if one add, that I lose the opportunity of
good many patients, who from the advanced stage of the disease, in which they were admitted, were convalescent in a day or two after each admission, rendering the observations useless as to the same trouble was compulsory, as though they were given of serice - the fact of being treated with such as those, perhaps, excluded my observing others which might have turned out productive of better results.

I may here observe in passing that all my observations have been conducted by myself - that in each of the diagrams as they are noted in the daily observation. I have preferred to have them remain a blank rather than trust to the observations of others, which might give rise to doubts in the mind of others, said accuracy. Hence, as I said in a former part of this essay, I can rely on the accuracy of what I offer.

Of these 11 cases of \( \frac{2}{3} \) then - 7 are Males 4 Females. Of the 7 males - 4 were under the age of 15. The remainder not above 30. Of the Females - all were young women under 23 years of age.

Every caution not Case 6 would not have been appended (owing to the unavoidable break in the
observations) but for the fact that the fever having been contracted in the beginning the 1st Day the disease was uncertain in the most satisfactory manner, I hence allowing a greater importance to be attached to the period of crisis. Otherwise it is certain the case is incomplete.

In all these cases it is clearly shown that there is a daily rise and fall in the temperature, the former obtaining in the evening and the latter towards morning. This is the rule under all events, the exceptions from invariable causes being slight. In these cases, however, where daily examinations have been made it will be seen that as a rule, the elevation is greater about noon than in the morning, and continues to rise towards evening. That the maximum temp. appears to be reached about 2 P.M. is not shown by the diagrams, nor that after midnight a fall commences. To prove this, would require much more frequent records and under the diagnosis very bulky. Trust therefore that my statement, that such is the case, may be received with credit as I should not for an instant advance it, had not first, to my own usually, satisfactory proved it to be correct.

On comparing the diagrams also (of the cases)
omitting Cases 1 & 2 because of coexisting pulmonary complication — that neither age nor sex appear to exert any influence on either the maximum elevation reached in the course of the disease, the day to which the highest temp. is reached, or the determination of the critical day — i.e. differentiation. For Cases 3 & 4 although showing a tendency for the disease to run a shorter course, both being of the same early age, the their attacks were comparatively mild as far as symptoms at any rate were concerned, which ought not therefore to be put on the same level with others of a severer type — with a view to individual determination & differentiation.

In none of these cases was a greater elevation reached than 103½, that was in one case, in the day only. (vide Case 7.). In two only did it amount to 105½ (vide Cases 5 & 8). The temperature ranging from 104° -105° - their maximum being generally reached about the 8th day of the disease.

The curve also appears to have kept at its highest (by which I do not mean the maximum point ever reached) from the 7th to the 13th day.

In 3 of the cases (1, 2, 4) the fever was abnormally low, and although the maximum degree of heat
reached by any of the 11 cases was not in either of these 3, till their maximum was greater than that of the majority of the whole.
In but one of the 3 cases where the highest elevation occurred (case 5) was there a previous curating. This was most marked on the day of maximum elevation.

In determining the critical day, I have reckoned the first day on which the morning temperature fell to a normal degree (i.e., a degree on either side of 98°). This might be objected to by some, acting on their opinions the period of convalescence. But in the nightly rise, after each critical fall, for some little time— as will be seen by referring to Cases 1, 3, 9, reached to the same level as when the disease was in full sway. And in all the cases—consistently above the recognized standard of health, it becomes some the objection is overruled.

Again, that the critical day cannot be determined sooner appears to me obvious, in the fact that some cases reach their maximum, and have from such time a gradual fall till the healthy standard is reached. This is especially noticeable in the 0 class, and the determination of critical day should be ruled alike in both forms.
The average of such critical days, appears to be on the 15th day of the disease — vide, Cases 1, 3, 6, 7, 10, 11. Although in all these a decided and marked fall occurred on the day previously (vide Case 6. Where temperature was noticed on the 1st day of rigor). In Cases 2, 8, it was deferred till the 16th day.

In Case 9, it occurred as early as the 11th day.

No fallary existing in this case.

In Case 3, critical day manifested a 13th. In both these cases a marked fall in Case 4.

So, apparently all in any of these cases. 1 in 3 where the wet sheet was applied no diminution of heat appears to have resulted.

The duration of cases. 1. On 6th, 7th, 8th days

2. " 8th, 10th, 12th " of disease.

11. " 4th "

The fall of temp. in all these Enanthematic cases, when the disease is proceeding is abrupt — conforming to Dr. Wetten's Remarks. Vol. i. P. 433.

I am also able to confirm Ronderhils's observation with regard to an abnormally low fall, during the 'depressed' state of fever, although he does not allude to its being specially noticeable in any one form of fever. Vide Wetten, Vol. i. P. 52. 123.

For in all these cases there has been a fall to more
Now a degree below the healthy standard (though not necessarily in critical cases) 95° having been reached in Case 5, 96° or a little above it, being present in 6 of the remaining cases—especially having reached 97°. Such a low fever appears to be only temporary. Slight in Case 11, a temp of 96° was maintained for 20 1/2 days. The cause of value of such diminution in quantity of further investigation. In none of my own recorded cases could I attribute it definitely to anything. Neither did it appear to be of any benefit or containing not indicating any treatment.

In all these cases but one (Case 10) did the temperature reach above 104°, in 2 however (3 & 9) not exceeding this. In no case was 105° ever reached. This will be found to differ with Dr. H. C. W. statement that in 98 the temperature always lies above 104°. Vol. i. p. 433.

In none of these cases does there any other than a favorable termination, or did any serious complication exist—Pulmonary congestion only being observed, that in but two cases. The maximum elevation compatible with life in this disease, therefore, I have not been able to establish.

It will be further remarked on looking at the
Tabulated sheets, that a certain relationship appears to exist between the rise and fall of fevers and the number of the pulse; although the relative rise of the pulse is less than that of the temperature.

The inferences to be drawn from the results of my observations of 20 cases are,

1. That neither age nor sex have any influence on the maximum rise or fall of leucos during the course of the disease.

2. That the application of hot weather does not exercise any influence either.

3. That a moist or dry condition of the skin does not materially alter the condition of the temperature.

4. That the temperature is higher in the morning than in the morning and commences to rise about noon.

5. That the maximum elevation reaches 105° but does not reach 106° (as correctly in favourable cases).

6. That the temperature rises towards its height from the 7th to 13th day.

7. That the fall is generally sudden and abrupt.

8. That the maximum fall may reach 95° but that the average is from 96° to 97°.

9. That each extreme fall is generally temporary but
10. That the critical is manifested by each fall of slump, and appears to be subject to variation, the 15th day being the most common—but that, an earlier crisis is also more frequent than a delayed one.

11. That after the temperature has once fallen to a normal point or below it, it does not rise again above the healthy standard, without some complication present.

12. That a distinct relationship exists between the rise and fall of the pulse and the variations of the temperature. That the pulse however is still a much accelerated as the temp. is elevated.

13. That the fall of temperature is generally more decisive at the earliest indication of crisis than in abatement of pulse.

14. That with the exception of the pulse nothing whatever was present to indicate any when near the time, the approach of crisis.
In the Typhoid fever - or gastro-enteric form of Typhus - my cases are more severe, although investigated with the same amount of accuracy. They amount to but 7 in all - none of these being females but me.

Of these 6 cases, 1 is over the age of 40 - December 20.

The rest ranging from that to 30.

In none did the temperature exceed 105°, this maximum was only reached in two cases both of which were exceedingly protracted - one of them Case 6, extending over a period of 32 days before amendment took place. The other Case 4, was accompanied by Hemorrhage, did not mend before the 33° day.

In 4 others 104½ was reached by all, while in the remaining one 103½ was the highest point attained. In 4 of the Total number there was an absence of Eruption but in the other 3 the Rash was typical. Complications struck in 3 of the cases viz:

In Case 1. Phthisis

In Case 4. Hemorrhage

In Case 6. Albinomia Pulmonary Operation.

Cases 4 & 6 were Hemorrhosed for a period of 31 & 47 days respectively.

The temperature kept at its highest during the
The whole of the second week, with the exception of the protracted cases 246 where this extended over a longer period.

With the exception of case 1, where it had been preceded as a complication the fall was likely proportionately less. The lowest temperature reached was between 97.4-98. In 3 cases, viz. 2, 5, and 6, was manufactured in them on the 26th and 24th-32 days respectively. In the remaining 4 that was no fall below lower than between 98-99. Such fall being noted on the 17th-20th-33rd-38 days respectively.

In most of these cases the term was highest in the evening, but in this particular instance, as before made to the diagrams, it will be seen that a greater fluctuation existed than in the rheumatic form of the disease.

A certain relationship is apparent between the pulse and the temperature, but not a proportionate one the exception being an acceleration above 100.

The state of the seat of the hemorrhage in case 4 unfortunately I am unable to furnish, not being a patient in the Clinical Ward, the fact was not communicated any where near the period of occurrence.
In Case 6, albumen was first noticed in the urine on the 22nd day of illness. The temperature coincident with this, maintaining its high standard, although in the ordinary course of the disease he might now justly begin to look for amelioration. The temperature remained high, without showing any tendency to fall, till the 32nd day; and although there was still albumen in the urine up to this date, there was throughout an absence of anything like pulmonary complications or intestinal symptoms.

On the 32nd day, the leuc. fell to its normal count, and no trace of albumen was found in the urine.

For some days following the fall, the difference between the morning and evening temperature amounted to 39.4 degrees. Still the fall being greater in the morning than in the evening, a certain degree of maintenance of temperature was noted. From the 37th to the 41st day, the morning temperature was diminished, but on the 42nd day an appreciable rise ensuing, a physical examination of the throat was made. No decisive evidence of pulmonary congestion discovered.
from the 49.  The short cough, with a steady increase in respiration, set in, to the 52d. The temperature increased. Unfortunately the microscopy was impracticable beyond the 54th day. I say impracticable for the rise at night was so great as to contrast in my mind, the letining of bone from fever. Understandably the Patient was up daily, and in a very few days left the inquiring. While all the time of her own statement alone were to be credited there was rapid regaining health & strength.

In Case I, where Pithen recorded the same condition of temperature is questionable as in the Case mentioned.

Yet a fall in the morning temperature to the normal standard, & a great rise toward evening—vide Case 1, 18th day, & friend.

The following facts then are manifested by studying the diagrams.

1st. That the maximum temperature reached was 105.
2nd. That the higher the temperature at an early period, the more protracted the course of the disease.
3rd. That as a rule the temperature attains its maximum toward evening.
4th. That the absence or presence of eruption does not
influenced the amount of temperature reached.
5. That complications tend to keep up the temperature.
6. That the fall in the temperature is gradual but sudden.
7. That the lowest fall is 97°.
8. That when subsolute complications exist, the morning temperature may reach the normal point, the persistent elevation only manifested in the evening.
9. That the temperature maintains its maximum during the whole of the 2nd week.
10. That as fall be a healthy standard issues before the 17th day, that in the average it takes place in the middle of the 4th week.

To differentiate between the temperature in the two forms of fever viz. O and O is by no means easy, or to any extent feasible, from the quantities of the facts furnished. For it will be seen at a glance, that the number of cases is not the same, nor is there any approximation between the quartets of the cases in the two different forms. Hence it would be wrong to lay down any results or may draw from comparison, as statistical facts; but we may hear them in
mind, to be confirmed or negatived by future investigations, when a greater number of cases shall have offered themselves for comparison. Such differentiation as we are able to make however appears to be somewhat as the following.

1st. That is accompanied by a greater elevation of temperature than 0.

2nd. That the elevation of temp. is of longer duration in 0 than in 0.

3rd. That in 0 an abrupt early elevation does not indicate a favorable course, or unusual severity of the disease. The contrary being the rule in 0.

4th. That the fall is more abrupt in 0 - in 0 almost invariably gradual.

5th. That a lower fall is reached in 0 than 0.

6th. That a greater variation is manifested in the daily course of temp. in 0 than in 0.

7th. That a more definite proportionate relationship exists between the Pulse & Temperature in 0 than in 0.

8th. That in 0 there is a greater acceleration of Pulse than in 0? 13. 0 does not appear to be more cases recorded of an early age than in 0.
That from the greater irregularity plus proportion relationship between fever and temperature, the notion of level temperature becomes of greater value in determining the critical day.

It was my intention to have investigated a quantitative analysis of the urine in those 2 forms of fever, with a view to ascertain what relationship (if any) existed between the amount of fluid and colic symptoms, particularly the rise or fall of temperature a subject which appears to me very worthy of investigation. Numerous difficulties, however, render such research in any part impracticable. There are a few other diagrams appended, which are distinguished as Case A, B, C, &c. No special facts however are to be drawn from them, as they are but single cases--still individual cases when accurate, are not to be despised. As from a collection of such facts results. But the cases cited present besides many points of interest.

In case A, which is one of rheumatic fever, the variations of the temperature in the disease in itself thrown, the observations extending over a period of 24 days. I have mentioned in a former part of this paper (Page 8) that an
Elevation to 105° is fatal (i.e. in this disease.)

In this boy. 104° was once reached - viz. on the 8th day of the disease. If this previous statement be correct then this case becomes of special interest - as showing that this fatal elevation to 104° may be approached to the utmost, & still be compatible with life - & give as an invaluable assistance to prognosis. This is a fact however that lacks & requires corroboration.

Case B. a case of Rheumatic Encephalitis is of interest - as the record stands over a very limited period - because as showing a much greater maximum rise & daily acceleration than exists in either of the specific forms of fever recorded. It will also be seen that as far as the observations go here is anything but a proportionate rise in the pulse.

Case C is a mild but positive case of Parinaud - & cannot here be taken as a type of the disease.

The disease - even if compared with a case quoted by Dr. Chitten Vol. i. P. 245. The case of his case is however not stated - & in any case there was no eruptive fever.
Cases D & E are simply preliminary exercises of the addition to these & diagrams of the tabulation in blue, into which indicates the variation of Respiration. In both of these cases a relationship exists between the rise & fall of the Pulse, Temperature, \&c., and in Case E, which terminated fatally, the day prior to dissolution a great comparative elevation of Temperature, increase in the number of Pulse, \& Respiration was manifested.

Before closing this matter, it would be only right to mention, in justice both to myself \& them, that the form of tabulation as seen in the diagrams is in idea of my own, but being an imitation of the form adopted by Professor Ringer of London, \& to whom alone I am chiefly indebted for becoming cognizant of the use \& value of Thermometric Observation.

In conclusion, I have but to regret that I have been unable to furnish a larger number of data. I can only offer them partly as an earnest of future investigation. At the same time, it may be borne in mind that if severity in December, they are reliable in matter.
I must also again express my opinion that it would be productive of immense practical and clinical good — the thermometric treatment, more calmly and recognized than it already is, especially when the matter of being so taken carried out — I hope that the Edinburgh library will be among the first of the British schools to acknowledge its value and put it into practical use.

Herbert Taylor

Edin: March 1864.
Dry cough last night - this day - X months -承办人手写。

Cough longer since frequent - X fractions - month

Headache - cough clearer.