Leaves from the Note Book
of a S. African General Practitioner

by

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Preface.

The notion of the "General Practitioner" at the Cape, as a far as it enables scope for careful scientific research, is a difficult one indeed.

The condition of Practice are in the majority of instances, so decidedly against any possibility for the systematic, close, and consistent attention, which reliable research requires, that it is scarcely to be wondered at that little or no work has been undertaken so far from the "African Practitioner." I need but mention that most of the latter appended were written at a time when I practised where I was not only always busy, but where at any moment I was liable to be called away to see a patient residing as far as three or four days journey away from my house.
The demands in consequence upon my time and energies were of such a nature, that all chance of close laboratory or clinical work was shut to me. Hence it is that I abandoned my original intention of elaborating a thesis bearing for its object some more special lines of work, and adopted the line Herin followed by me — It seems to me that the course adopted has not been altogether successful whilst medical progress demands the careful and close research, while almost alone is allowed to our fortunate brethren in the large and more stimulating centre of thought and action, it were the less demands proofs that work so done, receive recognition, and enter into the thoughts of the busy general workers, scattered over the globe.
It is with the hope rather of indicating the kind of scope, and kind of thought, which modern medical advance has rendered necessary to all engaged in the pursuit of practice, that with the consciousness of any work accomplished, that I offer the following imperfect efforts of mine to the consideration of my old readers.

The difficulties in preparing these notes have been many—isolated, without expert help, every leisure moment absorbed, it is no easy task to be equal to the responsibilities thrown upon one. It is hence with the difference born of the consciousness of imperfection that I come forward.

It will be seen that I have endeavoured to travel over wide ground.

In my first Paper I have tried to show the sort-of emergency
Which at any moment may arise here, which Emergency, which in this case has to be faced practically single handed, to which demand to the utmost degree, that Wokers at here should keep themselves informed of what goes on in Europe. This part of my paper I have made as Practical as possible. It represents not an early case of Cerebral Diagnostic Surgery, but as far as I know the first recorded South of the Line.

In my second paper I have endeavoured to show the kind of Speculation which a Consideration of Practical facts may lead to. I have allowed myself I know will well in this to Tr:avel into Region not only disputed, but also largely disallowed by Modern Research. I however none the less hope
for indulgence.
A cast-ast-aose upon the water oftentimes drifts, and if I have drifted, being cast alone from the direct influences of the laboratory, I crave at least sympathy.

In my third paper I have chosen a subject which is an indication of one of many important matters. Writing research in countries like this — which has I venture to think, many and wide practical bearings —

In my fourth and last essay I have added a small but imperfect research bearing upon an unlimited and unexplored S. African field.

The cast waves, lopped up in the Cape flora, are as yet unknown and incomplete as my notes are; they will at least...
With not the common diffuse atmosphere of
intestinal indigestion, the kind of material
which has been ready at hand, for
development.

The Pharmacology of the body I
describe (Potascan) | I have not investigated
yet, but I shall hope to make that
the object of a future research.

For the completeness of my paper
I am indebted. The time absorbing
energy absorbing demands of practice
in the African clear but little room
for work of the sort. Scarcely any
two days of my experiments were
undisturbed. Without innumerable
interruptions, indeed had it not been
for the kindness of an assistant,
Dr. J. Maring, who often watched
things for me when I was called
away, I should probably have given up my
search for (Potascan).

J. H. L. B.
S. Nov. 89.
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I.

Note with Remarks, on a case of
"Loculated Meningitis with Effusion",
Complicating "Otitis Media", Acquired
Successfully treated by Sphintering
and Evacuation of the Fluid — by

J.H. Meunuy, M.B., M.C. M.R.C.P.S. L.
I. Not on a case of Localised Meningitis with

  Effusion "Complicating Otitis Media, diagnosed
  and successfully treated by Draining and
  Evacuation of the Fluid.

I saw my patient (J.W.) a boy of 5 yr. (10) first
on the 24th of May 1868. He had had Diphtheria
in 1881, followed by Scarlet Fever, and a purulent
running from the left ear.

This lasted almost a year, when he developed
what from the description (he was not under my
charge then) must have been an abscess over
the Mastoid. This was lanced and
he recovered completely. About a month
before I saw him, the left ear commenced
to discharge again, but there being nothing
out of the way, it was allowed to go on.
At the end of 2 months it showed an
Purpur in diminishing I was sent for.
When I saw him I found him suffering
from a copious discharge, offensive and
purulent from the left ear. His temperature
was 101.6 - hi pulse 112 - There was a
was a slight- symmetrical parasthrea. Hearing
was impaired, but- Bone-conduction & tone
of the tuning-fork, was quite good.
Diagnosing the case as one of ordinary
"Putrid Fever" I ordered the man to be gently
irrigated two or three times a day with warm
Boric acid solution. The bowels having been inactive
for a day or two, I ordered a bland enema.
On the following day I found him improved.
The temperature however was still raised, the
thermometer still registering 100.8 at
11 o'clock A.M.

Three days after he complained of
headache. In enquiring I found that
he had complained of slight headache before
I was sent for, but- it had gone away.
It had attracted no special attention. On
ailemnence in the 16th May it wax more
severe and localised definitely over an
area extending from the left- ear forward,
and upward, to the middle of the forehead.
The temperature was 101.6 Pulse 108.
The headache was paroxysmal, but at times
time, extremely acute, and during the following days, it increased violently in intensity.

As it increased in severity, the temperature more or less steadily fell, and with it the frequency of the pulse rate diminished. On the 23rd May the temperature was 98°. Pulse 66. The boy was drowsy, every now and then he started up with a low moan. When the pain in the head became very severe, the pupils were equal. Considerable pain was elicited on percussion immediately above and in front of the external meatus. He was counting incessantly.

The breathing were intimately connective. Even with aperients, having no effect. He had lost flesh rapidly. The breath was foul, the tongue dry and cracked. The teeth were covered with sordes. He lay upon his right side with the hand under the head. The left eyelid seemed at times to droop slightly when he was crossed. An attempt to procure an ophthalmoscopic examination was not successful in his altered Prostrated condition.
There was no expectorated tenderness, the discharge from the ear was copious.
The left jugular vein was normal. The urine was free of albumen. There was absolutely (with
the exception of slight left Acute Phthisic" uncertain) no motor disturbance anywhere.

Regarding the ear, now on one of
localised meningitis. Induced a blister
behind the ear, and another on the scalp.

This condition however remained unchanged, indeed every one of the symptoms became
gradually worse. The constant remained
as incessant—that I was obliged to resort
to nutritious emulsions to sustain him.

By the 25th May, the Temperature
was persistently paroxysmal, the pulse beats
varying from 40 to 60 per minute.

The constipation was so alarming that action
of the bowels seemed almost impossible to
be induced. He was wasted to a

By the 29th May, the head became seriously
worse—Taking into account the
the continually subnormal temperature and pulse, I strongly suspected Intracranial
Effusion - I therefore continued my
Counter irritation over the scalp, in the hope of
being still able to influence what might
after all be only a meningitic attack.

The absence of anything approaching

to rigor, made the existence of Pet, uncertain.

One thing however was clear - there
was compression of the brain, and comparing
the condition with that of the few cases
published that I could gain information about,
as well as taking into account the absence
of motor disturbance, the part affected
seemed to be almost certainly the \textit{Insular}
Sphenoideal Lobe - Next day the boy
was so ill that I feared dissolution at
almost any moment. A point had
been reached when it was perfectly
evident to all concerned that death
must come if no change intervened -
that being so, I decided to interfere
Surgically. I accordingly asked
my colleague Dr. Herman Ate and Selke, to see the case with me. This they kindly did. After consultation we decided that it was advisable to perforate the Mastoid Antrum first with a view of procuring if possible a free drainage of the middle ear, and also of cleansing out and asepticaizing as far as possible the part of the ear affected. So here we two therefore perforated the mastoid then and there,打通 the middle ear thoroughly through the perforation made with the Convonie Jullmilt Solution (1 in 3000). The state of collapse after the operation was serious. I quite expected to see the boy pulseless, but I asked my colleague to see him again next day with me. This they did, so decidedly was the patient worse that we unhesitatingly agreed to open the Cranium. The temperature was 97.6°, Pulse 58.
Operation - June 1st. 11 o'clock A.M.

The middle ear was as carefully as
possible aseptically with Conino
Dublinate Solution.

The scalp was shaved, carefully cleansed
and aseptically - Alcohol, Ether + Chloroform
was used as an anesthetic - I made a
Crucial Incision including Skin, Muscles and
Periosteum, & applied the point of a half inch
Triglaive at a point an inch above, and
slightly behind the Auditory meatus.

This point was selected as both likely to bring
out the Temporo-Sphenoidal lobe, and
as being immediately near the seat of the
greatest pain. It was moreover the
point which in a case that corresponded
very closely in point of symptoms with mine,
(see my former case) had yielded a very satisfactory
result.

The bone was normal - The Wires were
Normal. As soon as the line of bone was
removed, we had at once ample evidence
of the Tension Existing within.
The Dura Mater, both relaxed Brains, at one
bulged into the occipital cavity and was
forced considerably above the level of
the divided scalp — So great was
the tension within, that it seemed as
if the Dura Mater were going to give way
at almost any moment. I incised
the Membrane freely, when considerably
more pronounced bulging of Brain Tissue
occurred.

I now shored the largest Tissue
and Canula contained in my aspirating
case, attached to an aspirating
Exhausted Aspirating Bottle

\[ \frac{1}{2} \] Perpendicularly to the Surface, for
a distance of quite two (2) inches into the
substance of the Brain — Nothing flowed out.

\[ \frac{2}{2} \] Backward, Upward and Inward, in
the direction of the Sylvian Fissure — The
needle had just penetrated about \( \frac{2}{2} \) to \( \frac{2}{2} \)
inches, when there was a rush of turbid fluid
into the aspirating Bottle —

About \( \frac{1}{2} \) to \( \frac{3}{4} \) oz. was drawn off — Unfortunately
Unfortunately by an oversight the fluid was thrown into a vessel containing some normal barium sulphate solution, before it was measured, so that the exact quantity could not be determined - I am however certain that \( \frac{1}{2} \) oz is decidedly under the mark. I now moved my needle gently about in the hope of procuring more but with the exception of some broken down Brain Tissue nothing more came.

The object of my search had been found - I however wished to make sure that no part likely to be overlooked had been overlooked. I accordingly passed my needle forward - and inward in the direction of the Frontal Lobe, for a distance of two inches. But nothing came through it.

I then moved in the direction of the Temporal Bone, until I touched the bone within the skull. Here also
also nothing was found.

It will be seen that my exploration was thorough, and included practically the whole of the Temporal Sphenoidal area. As the fluid evacuated was not Pur, but Inflammatory Effusion only, I did not put in a drain. My reason for not doing this was threefold—

1. If more liquid formed the tract by which it could find its way to the surface or free (I had broken down a considerable quantity of Brain Tissue).

2. The relationship of the skull-marrow, the convexity of the Fluid to the Temporal hole, was such that in the recumbent position lying could readily take place in the direction of the opening made.

3. If then formed, I knew where to go for it— it could at any moment— instant— a drain having shown an opening through which to do this.
As will be seen, the further progress of the case amply justified the wisdom of this procedure. During the time from
leaving the brain, Dr. Keenan tells me that he noticed a marked dilatation of the left pupil. This resolved after the
operation, and the pupils were practically equal almost immediately after.

The pulse almost instantly increased in frequency.

The wound was treated with Peroperineum Acid, & dressed with Conviron
Sublimat Prepared Ginger. On being placed in bed the patient was naturally
much collapsed, but he soon revived.

Condition at 6 P.M.

Still Brown, Pupil Equal.
Pulci out—his hand on being requested to
do so—has made signs for something
to drink, which he retained. For many
days nothing had been retained.

Pulse 76—Temperature 98.4.

For days there had been subnormal Pulse and
and Temperature) — Higher heavily occasionally. —

As removing the bleeding formed, a large amount of oozing, the bleeding was completely stopped, so it was taken off, a considerable quantity of fluid flowed out. There was considerable fluctuation in the exposed surface.

June 2d 11 o'clock A.M.

Slight early heat of the right

Made signs for drink twice, breath recovered, the first time for days. — Had kept in Thursday three times and returned it. — The vomiting seems to have completely stopped, refreshes equal.

The tongue, and mouth are drawn decidedly to the right side. — There is evidently Asphyxia present.

Right heavily, occasionally — So since, decidedly less during, but has still some strain in the head. — The acute

Pneumonia has now entirely ceased, there is still much oozing of fluid.

The brain has now quite recovered
Into the skull, all signs of bulging has disappeared. There is marked improvement throughout.

Pulse 84. Temperature 99.4.

June 3rd. 10 o'clock A.M.

Able - well no pain, no vomiting. In middle of night - said with difficulty: "Want to eat", and - are being asked what he wants. Could not name it. Said after many attempts, "What Pa brought." His father had brought home some oranges that afternoon and showed them to him. The aphasia is marked - Tongue cannot move. First breath disappeared. Sounds from teeth gone. Mouth & tongue still drawn to the right.

Pulse 74. Temperature 98.8.

June 4th. 10 o'clock A.M.

Aphasia improved. Tongue and mouth less drawn than before.

Sat up to eat - an egg. Left-ward. Movements almost - quite gone. How'd more.
June 5th, 11 o'clock A.M.

Dongue better. Aphonia better but still quite evident. Speaks with great difficulty but cheerfully.

Left eye & adjacent side of face oedematous, no reddening of skin. No conjunctival congestion. Otherwise marked improvement.

Dysesthesia in hand & foot, had had at his own request a small elect. The wound isgranulating well.

No reddening near it. Discharge from it still free. There is still some discharge from the ear. Mortier perforation.

The powdered arsenic acid was omitted from the dressing. Todeform need instead.

Pulse 80. Temperature 98.4.

June 6th, 10 o'clock A.M.

Bowel motion well. Appetite good.

Oedema of lid & face very much less. Speaks now with more ease.

Tongue & mouth still somewhat drawn.
Left well, looking better only at 2 A.M.

Pulse 84 — Temperature 98.4.

June 7th

Improved in every way. Is making flesh rapidly. Tongue clean.

June 8th

Playful tonight. Has asked for one

Aromatic in his bag. Left well, appetite

Greatly improved. Has now perfectly

An affected side.

Pulse 86 — Temperature 98.4.

June 11th

Growing flesh rapidly. Looks

Health. Growing rapidly. Well. Still having

From sun still continue but in less.

The Mustard perforation is evidently

Healing fast.

Pulse 91 — Temp. 98.4.

From this time there is little to

Chronicle. On the 11th June allowed

him to get up. The Aphasia was

Then practically gone, and the tongue

1 month so slightly drawn, as hardly
Hand to be noticeable.

Wound cicatrizing rapidly.

June 30th

The wound healed in every way quite well. Affected ear.

Pulsating discharge. Hearing good.

Bone Conduct. Normal.

No optic neuritis of left eye.

Remarks

The number of cases of Central affection complicating ear disease, successfully treated by operation are as yet extremely few. I think Jam Cond. when I say that five only up to this time have been recorded.


One by Dr. Ewen of Edinburgh. Lancet March 26, 1887.
One by Barker - B Med. Journal, April 7th, 1886.

One by Torrie - B Med. Journal
March 10th, 1886. * +

This being so, it seems to me almost an imperative duty that I should record as fully as possible the particulars of my case. No one will deny that the subject is sufficiently important to justify all the attention that can be directed to it.

Analyzing the cases above mentioned we find that -

First - In Ewer, & Barker.

* - Case by Schomburg, Meckel's. A particularly interesting one, really belonging to this series, as much as in both internal and external, it is so that localization of the seat of mischief can not necessarily the risk of operation reduced to a minimum.

+ - Since writing the above, Mr. Ewer addressed before the recent meeting of the B. Medical Association at Glasgow, has come to hand - From this it appears
Paper case, there was almost complete absence of nervous symptoms, with subnormal temperature, after the first onset of the acute attack of the disease and with consistently slow pulse. There was dull aching in the Temporal Region, slight vomiting only. The Reflexes were as follows:

One and a quarter inch behind, and one third inch above the Centre of the Lateral wall of the ear, at a point corresponding to the Infrasecond posterior angle of the Parietal Bone, close to the Squamosal Suture.

I appear that in a few more cases, and of none of them did a record exist at the time of my examination, and indeed most of them are subsequent in point of date to mine. Under any circumstances the my case is sufficiently early to justify careful record.
B. An abscess was found in the Temporo Sphenoidal Lobe.

2° In Greenfield & Land Case - There was severe headache, vomiting, heaviness with much browning, walking, wanting, equal pulse, no motor disturbance.

Punct of the Neuritis of left eye. The trephine was applied 1/4 inch behind the external angular process, and nearly 1 inch above the zygoma.

An abscess was found in the Temporo Sphenoidal Lobe.

3° In McEwen Case the symptoms were almost absolutely similar to mine.

The trephine was applied where I applied it. An abscess was found in the Temporo Sphenoidal Lobe.

4° In Barker's Case, there was first violent decreased headache in the Temporal Region. With Swelling hyperesthesia of the side of the head affected.

This was followed by vomiting. Then obscurities with musical coma.
followed by intensification of pain.
Later there was partial left hemiplegia,
with twitching of the left wrist, and some
rigidity of the arm, together with
hyperactive tendon reflexes.
And ankle clonus symptoms. Two
openings were made — The first in
the region of Rolandic Fissure — 1/2 inch
above the Sylvian Fissure. The second
1/4 inch behind the centre of the hsemispheric
depression and about the same distance above
it. Before the operation the case
was diagnosed as one of localized
meningitis with inflammatory
exudation, and these were found to be the
cause. The inflammatory process
had accumulated among the convolutions
of the deeper part of the Sylvian Fissure,
between the Island of Reil, and the
overhanging sector of convolution,
and pressing the latter backwards
against the skull.

In Farmer's Case. There was drowning.
Brownin', with pain on the left side of the head, forehead, & back of ears, with fear of photophobia. Vision unaffected. No loss of sensory functions. Speech affected. Wrong words being used. Inability to read correctly. The right angle of the mouth acted less expressively than the left. Pupils equal, + contractile. Pupils normal. Visual field normal. The left ear was discharging. A tender spot on the head 2 inches above + a little in front of the external occipital. No vomiting, no convulsion, or fibrillary disturbance. The lesion was probably abscess in the anterior third of the temporal. Sphenoidal lobe + abutting on the fascia of Sylvius. The Kaphlein was applied on the side of this abscess. No abscess was formed where the kaphlein was applied.
I symptoms most closely with the swelling over the accumulation of inflammatory exudation. Could not have been far from the actual spot, at which Barrie found pus, in his second case. That I had to do with a localized meningitis was clear, and that the seat of effusion was in, or adjacent to, the deeper part of the Sylvian Fissure, the point at which the aspirating tube penetrated, before I found it, proved amply.

Unlike Barrie I had no distinct symptoms, and there were no indications of pressure upon the overlying Parietal Convolution, but the track of the effusion no doubt differed slightly from that in Barrie's case, at the Parietal Efercered Point having been entirely in the direction of the Temporo-Sphenoidal Lobe. Taking all things into account— I think it almost certain that I had to do with a meningitis,
measur'd, which spreading from the
pyriform recess as a focus, affected
the meninges, slipping between the
Dumros' sphenoidal convolutions, were an
absolutely localized area, & to determine
the effusion between these convolutions,
& the pressure upon them entirely—

One in two interesting
points, are raised by any case, & I
may at be allowed briefly to refer
to them.

In the Edinburgh Medical Journal
for May 1st June 1867 MacAlpine in
an able monograph, discuss the
question of the cerebral complication
of ear diseases—Amongst other
things he says: "In many cases of
intracranial abscess, both pulse
and temperature are at some period
after the onset of the acute symptoms,
persistently subnormal. —
Indeed when this condition exists, as
it has existed, I should look upon it,
evidence as rendering it more probable
that a well defined collection of pus,
rather than a diffused meningitis or
Phlebitis, is the cause of the head symptoms.

My case proves that while
Mastoid is right, in regarding
Abnormal pulse & Temperature as
likely to be associated with a localized
& defined Collection of Fluid, that
that Fluid need not necessarily be
Pus—

With reference to the condition of the Ear,
Mastoid points out that normal
Bone-conduction, should when present
indicate an affection of the Cerebrum
rather than of the Cerebellum; this was
amply borne out by the present case.

The fact—however that there should
be any time in normal Bone-conduction
in these cases, is one which is
remarkable when we consider that
the Internal Ear is almost immediately
in contact with the focus of disease in
in the middle ear, & therefore, might—reasonably, be expected to participate in an extention of the disease, thus of function, some times posterior of the brain, deeper in.

A third consideration suggested itself, upon the treatment of ordinary Meningitis. Is it possible as in the case to diffuse, care a meningitis by infection, septic in origin by operation, Should surgical interference not offer a fair chance of success in cases of idiopathic meningitis when acute—symptoms arise?

For my part, I must confess that I shall in future with the experience of this case be strongly tempted to open the cranium in cases of meningitis, where they reach a stage at which death seems imminent, provided of course that in all—hand sufficient data for localization of the part affected— I can see only benefit likely to accrue from a relief of tension, and I fail
first quite confident— that in favorable case, success will crown the effort to
afford this — McEwen does I believe attempts in one case at any rate (three
day or more but I do not know about them) to operate for meningitis. His
patient succumbed but— see like prime indicates that we are justified
in making the attempt.

The occurrence of the operation
of Aphasia with drawing to the right
side of the mouth & tongue, is both
interesting and instructive— There can
be no doubt I think that in passing
my probe forwards in the direction of
the frontal lobe, I must— have actually
gone past the primary Sphenoidal
boundary into the Left ascending Precentral Convolution & created a destructive
 lesion in Verne ‘Oro-motor’ Centre
(Broca’s region). The occurrence of
Aphasia with Left Lingual Facial
Paresis at any rate suggests probably
Probable proof, if any further were needed, that Ferrier is right— in regarding these conditions as associated phenomena, dependent upon disturbance in brain convolution. It is true I quoted in other directions. They have caused a lesion elsewhere which may account for the result, none of the explanations I made however seem to me in the least degree to bear some near any part— which we can associate with the function of tongue & teeth phenomena. The result produced was most certainly consequent upon the operation, as, although the boy was too young before it, to allow a diagnosis of the presence or non-presence of aphasia, he was not so conscious that he could not put out his tongue, to the drawing to one side was distinctly present— till after operation. If the tongue movements were normal before, it follows that there was
was probably also in aphasia present before, as much as Frieze had distinctly demonstrated, the two conditions to be more or less associated phenomena.

It is gratifying to be able to record the complete disappearance of the headache caused, and the resumption of an absolutely normal cranial condition, a result due it seems to me most probably to a union of nerve tracts in the region named, which were disturbed and healed during my working operation.

The point most favourable for applying the trephine seems to me worth a word. In all the cases recorded, the collection of fluid erupted in a near the supra-sphenoidal lobe. Considering the thorough completeness with which the opening made by me enabled me to explore the whole of the region, considering further the facility for...

for draining at this point, from the
head so frequently affected in these
cases. In the intracranial disease, I am
quite of opinion, that this is an
favorable a point in any that could
be selected. Special conditions must
of course, always demand. Special line
of procedure. But, the point in
question, seems to me one which would
have amply satisfied, the necessity
of all the cases, quoted by me, in
addition to this. Had it been omitted
in Raben’s last case, it would
certainly have obviated the
necessity of a second intracranial
opening.

The remarkable rapid, continuous
improvement which followed
operation, which set in the moment
into cerebral tension was relieved in
insolvent. - Looking at the day
6 or 8 days after operation, it was
was difficult to realize that he was the
same patient of a week before, when
hardly a single function of the body
was unhampered, 
that death threatened at almost anytime,
when moreover, destruction was
making such rapid strides, that
he had in an incomparably short
space of time, been deprived of
almost every vestige of superficial
line - never before in my
experience, have the relationships of
effect to cause, 
removal of
cause, to removal of effect, been
so strikingly demonstrated.

That in cases like the present it
would be possible to procure, by
through a condition of sepsis,
is, I think extremely satisfying, more
particularly as in this instance I
had not only to contend with the fact
of risk, from purulent discharge from

the care, an inch from the direct line, but also had to fight in surrounding
which were by no means favourable to

It is a source of regret that a
Consciousness of many imperfections,
is forced upon me, in my record.
It must however be borne in mind,
that my case occurred, as in hospital, but in better class private
practice, where many difficulties not
only presented themselves, but where
full records were through many stages
almost impossible. It must be
further remembered that in Practice at
the Cape, Specialisation is unknown,
and every practitioner is compelled
to be his own Pathologist, Aunts
Specialist, Physician, Surgeon &c.
Under these circumstances,
Imperfection of record, will I hope
be regarded as excusable.

The gratifying fact is, that
it is possible to succeed under conditions of great disadvantage in treating a condition which is
while lying on the threshold of a new region common to Medicine & Surgery,
so complex, that few anywhere will be so able to direct themselves
of a feeling of dismay, at approaching it.  

S. H. H. March
II.

Facts and Inference from a Small-Pox Outbreak Which Occurred at Worcester, Cape Colony, during the Year 1884.
Facts & Influence from a Small Pox Outbreak which occurred at Worcester, Cape Colony - during the year 1884.

Explanatory - Worcester is an inland town situated on the direct railway line 109 miles from Cape Town. In the beginning of 1884, small pox appeared in Cape Town and spread from a case throughout the colony, with the exception of Kimberley - the centre of the Diamond Industry. At Kimberley in the following year an extremely virulent epidemic arose. Several cases were brought down to Cape Town, but no spread at all occurred from them.

Robertson (a place mentioned in my work), is a small inland town situated about 30 miles from Worcester, not connected by railway with it. The case occurring in the Robertson
District (mentioned in my report) developed on an Isolated Farm - He spread all occurred from it.

In 1847 I held the appointment of Government District Surgeon, Vaccine Officer for Worcester, so that I had every opportunity of knowing what were going on, of observing from a point of vantage the behaviour of the Epidemic.

Small Pox appeared in Worcester, in the person of a Woman, who had it in Natal, brought the illness with her from Natal. (This was near Boxed). This occurred in the latter part of June 1847. In her house were:

1. The husband - Vaccinated when a child.
2. Four children previously unvaccinated. The woman died, her husband escaped the disease, the four children all contracted it, one of them also falling a victim to it.
The next case was in a family, living
next door to the first —
Here a child was attacked on a
Friday — having been vaccinated some
days previously — Contact of this
child with the previously infected
family was traceable — Immediately
on being called in to see the case, I
vaccinated

The father & mother, previously vaccinated
with their son — The mother & father of the patient, varying
in age, from Infancy to about 14 years,
— Three of them, previously vaccinated —

The patient himself recovered
the attack, being an extremely mild
one, all the remaining members of the
family escaped, notwithstanding that
some previously, unvaccinated, had
already been in contact with the
affected member, before they had been
vaccinated — Contacted with the
four children in the first House, their
result was striking, and an unqualified
indication of the power of vaccination.

As will be seen hereon still more
remarkable proof offered itself.

The next case was two colored
kids living next door to the originally
infected family, with whom contact
was traceable — Both had been
vaccinated, but are only a few
days before the disease showed itself.

Having been like the previous case
infected previous to vaccination,

Both had serious attacks of confluent
smallpox — In both unlike the
unvaccinated originally
infected family, Secondary fever
remained away, and convalescence
commenced on the ninth day, a
result attributable we doubt to the
influence of vaccination.

A child about 200 yards
from the nearest point at which
the disease had fixed itself was
attacked — Vaccinated on Saturday.
Saturday. She fainted on Tuesday followed, she developed a serious attack of confluent smallpox. Here as in the two previous cases, the vaccination though performed only two days before the disease manifested itself, developed sufficiently well to prevent secondary fever, and to save I confidently believe the child's life. On the ninth day, although up to this time there was great exhaustion, the child convalesced and recovered rapidly... In the form with this patient were 1. Mr. P. Parent - previously vaccinated 2. A sister - mother previously vaccinated 3. Mr. Mother (one an infant in arms) previously unvaccinated.

On being called in I had some doubts about the nature of the case, but to be on the safe side recommended vaccination of all the inmates of the house.
I had no lymph used. I could pursue done till last day. As soon after this as possible, vaccinated all, and here again success crowned the effort to prevent. All escaped. In order to explain what happened here, let me draw particular attention to the sequence of events. A child vaccinated on Saturday, developed smallpox on Tuesday following. Had a bad attack, but was convalescent on the 9th day, the time at which an uninfluenced attack would have been at its worst. The previously vaccinated members of the family escaped. The previously unvaccinated member (although vaccinated only the day after contact had been established with smallpox, in other words after infection had in all probability taken place) escaped, notwithstanding that there was throughout constant and unavoidable intercourse.
with the patient. I may mention in passing that this patient developed a few months after her attack of small pox, a severe attack of chicken-pox, a fact which seems to prove a decided difference, not only in the degree of intensity, but also in the nature of the poison causing these two conditions.

A most occurred a severe case (fatal) in a child previously unvaccinated. In the house were four parents. Previously vaccinated, both of them escaped. And now the disease commenced to flag. Fifteen or twenty days elapsed without fresh cases. When one more occurred, the case was proved to be imported from Cape Town. Two more cases finish the list. The outbreak lasted from about the middle of June to the middle of October.

And now the question arise: How did it come about that the spread
Spread was so slow & so comparatively slight. Why was the disease stamped out so effectually? There can be no doubt that a spread was inevitable & the subtle & unmistakable way in which the poison infected is too well known. It was only too well illustrated by the extremely virulent & fatal epidemic in Cape Town during the same time to leave room for doubt, with regard to the extreme risk we were running. What has gone before will already indicate what our sheet-anchor was, & in what direction our concentrated our energies in order to create a condition of safety for our community. Vaccination was absolute & unconditional safety. As soon as the disease appeared isolation of the affected cases was, of course attempted. This we doubt & did work, but there can be no doubt that to some extent their diminished
the risk of spread, it would have been wrong not to have attempted as far as possible to carry this out. While however recognizing the value and help derived from it, I hesitatingly say that it was to the assistance the public gave the medical men in coming forward to be vaccinated, that we were enabled to rid ourselves of an intruder that threatened us so seriously.

* In all cases, almost vaccination was affected from arm to arm. The people were wise enough to see that they were only increasing their own risk by insisting on being vaccinated at home, and all came up will to the

* In Europe the risk as it appeared to us can hardly be appreciated, as smallpox had been heard of for many years, & the whole community were practically (there being at that time no compulsory vaccination law) unvaccinated at the time of its appearance.
in different places my colleague and I had appointed, with the result that in a
remarkably short space of time, the whole community was placed in a condition
of security. The disappearance of the Epidemic was as rapid and spectacular as
at its appearance. I shall I hope be pardoned if I have endeavored to what in
follows shall be the influence of vaccination. Medical men
in it may properly be urged are quite
agreed as to this. The unusually
sharp definition. Isolation of the Epidemic
was however so striking, and the special
knot of a practically unvaccinated
community comprising at least
some thousands of inhabitants
the present that the experience deserves
a record. I have also another reason
for this record. As can be
readily imagined from what I have
said above, the experience afforded by
the necessity for a safe vaccinating,
a rapid as possible from thousands of individuals, made it also an inevitable necessity that I should know pretty soon subject operated upon first. And here I had an opportunity of making an observation which, while it at the time made but a slight impression, seems to me now that I have reflected upon it to be very remarkable.

I refer to a fact— which is not only I, but also all my colleagues who during the same time were similarly occupied, verified absolutely. Again and again have I discussed it with them, so that it may be accepted as an observation which admits of proof— few if any cases came back to be revaccinated. In almost no case did vaccination fail to produce effect, it was only, that, but in a very large majority of cases, the effect produced was quite 

Almost everywhere I had instance of an abundance of local and constitutional disturbances in those vaccinated which I could at first regard as quite disproportionate to what I expected. It must be borne in mind that a large number of cases had been vaccinated before (a long time before it was true, but still as much on their arm testified) so that one might have reasonably expected a considerable percentage in them of failures. But—still it is only the most of them developed pox vaccine putridus, but together with those previously unvaccinated they developed an amount of constitutional disturbance which was remarkable. It may be urged that in many cases in the burning stools & crumbly work...
For necessity at the time, the lymph employed might have been the cause of the but this was not so. I am supremely careful to cleanse red only, but had absolutely no fear of Empipros or other antihemorrhage, which might be attributed to bad lymph. So far as I had a return to normal health in the unusual time. It was quite impossible for me not to know of cases that were wrong, because at the time I was the first- and which the whole of an little initiative would revolve. The condition of "care" was so great that I had solicited enough from all sides. There was no time for assistance in a hundred and one trivial aches which fight had converted into a supposed smallpox and to render it certain that I should have been
Approached from another, if anything had gone wrong, any wrong. My own, thy colleague. Observation in this respect may therefore be accepted as sufficiently verified to make it reliable. If then the doctrine be correct or incorrect, what, it may be asked does it imply?

My answer is: That some cause, internal or external (perhaps both combined) must have been in operation at the time, to render the susceptibility to the smallpox carrying virus, particularly great. I believe, that this cause really determined the epidemic which threatened in so many places at once, and which spread in the population, center with a rapidity which at the time was alarming. In other words, I believe that while smallpox introduced from without, might
eight at a time have died out; at this time smallpox individuals be prepared for the reception of the poison that it at once took hold of the community and spread right & left.

Another line of reasoning there could be advanced to me in this,

If at any time an extraordinary susceptibility to diseases can arise, if condition can occur which favours the development of any specific disease form, we should always go to points of contact to explain the appearance of such diseases as smallpox, & the allied symptoms there.

As knowledge at present stands there are probably few who would be prepared to entertain the possibility of the
dezero* origin of smallpox, & its allies.

*By dezero I do not wish to infer spontaneous generation. I have ideas from some source, but rather the assumption of specific property for in one or other direction from a condition of non-specificity —
That a disease like smallpox or scarlet fever might be able to arise spontaneously, where before it did not exist, where importation from elsewhere is not present, would be to the ordinary mind not only an unscientific assumption, but would be to many a startling contingency. It need however in no sense any means induce any kind of additional anxiety. In the case of smallpox, if we knew of Vaccination to be true, as we know it to be true, or what I have written above proving to be true, then we have at hand a ready means of rendering an individual safe absolutely. Can we complacently contemplate the possibility (as far as this disease is concerned) of no one being in the world from where we have not arrived at an entirely satisfactory means of prevention, as yet? The possibility of a disease once digested can have the effect that if stimulating fresh attempts to arrive at such means —
I frankly freely admit that the difficulty, may even at present the impossibility of proving any theory which declared the origin of the Anti-Tetan, further than they do now produce and indeed it is because it is difficult to prove that I have limited as it.

Isolated as we are in this country, we have special opportunities of determining the mode in which disease appears to spring up amongst us. Communication is so many instances difficult between different places, and we have a thinly scattered population spread over a very considerable area of country. This being so it seems easier to have the origin, progress, prevention, and arrest of epidemics such as those of the blight, so called Specific Fever than would be feasible in crowded towns or thickly populated countries. At any rate planning for the present the possibility...
Of the spontaneous origin of smallpox and allied diseases, we could do
much if we systematically recorded
our observations in this country towards
settling a question which not only
regard as extremely difficult and
complicated, but also of extreme
importance.

Arguing from analogy, it is not
unbelievable that a disease like smallpox
may originate de novo. There must have
been a time when it first arose, and
if it originated once, it should never
be possible for it to originate again,
given the same combination of causes
and circumstances. Hence, that
originally existed. Many times I
have had opportunities of witnessing
the sudden appearance, and what is
quite as remarkable, the sudden
disappearance of measles, chicken-
pox on isolated farms, isolated
villages away from other habitations, and
in many cases, though I have taken every possible trouble to find out whence they came or how they originated. I have been unable to account for their appearance.

It cannot too clearly be reiterated that in British Central Africa, an isolated farm means something which is really absolutely by itself, and away from all possible contact almost—Inhabited as a rule by a limited number of people whom movements individually are often traceable in past accuracy, data exist. Such as exist in few other parts of the world, of arriving at conclusions as to location, for disease.

It is exactly the difficulty I have often experienced in accounting for the sudden appearance of the appearance of those at such sites, where no apparent contact could be proved to exist. Coupled with my conviction, results justified above, that first led
me to indulge in speculation as to the possibility of the theory I have indicated.

I have before me at this moment a Blue Book containing the Government surgeon's reports for 1862. In the report of the District Surgeon for Robertson (with explanation at beginning of the paper) I find the following passage.

"Another interesting occurrence was the outbreak of smallpox, six miles from the village of Robertson on the farm "Rood-Hooptle" in July last.

"The most careful investigation has convinced me that the outbreak was a spontaneous one in a coloured boy, about 15 years old, who was herding the cows of Mr. Hendrie Colyn. This boy had not left the master's farm for a considerable time, and had not come into contact with anyone except the members of his own family, of whom none had been further than to the"
villages of *Werteking* (where no smallpox occurred at all during the whole period of the tit-first) for the last two months previous to the making it appearrs themselves. The nearest smallpox case occurred at *Worvete* 30 miles distant — the but where the disease broke out was inhabited by from persons of whom only one escaped infection.*

This case bears me out — in my assumption — of course I am aware that contagion might have been carried by the boy in hundreds of ways, by letter, by clothes, by many other ways, & I admit if but the probability, that this may have been the case —

If this be so however, it is at all events strange and unexplained that smallpox should have been carried to the isolated farm, and

W — to the village of Werteking, where
In Dr. Milligan paper he hint at a speculation not dissimilar to that indulged by me, I however may claim at least independent speculation, as I read some time before his paper appeared, a paper before our local medical society substantially expressing what I had endeavored here again to argue.
Much from constant communication with the ether would result.

It may be thought that I am singular in advocating the shortness of the theory that correctly represents it. In the "Glasgow Medical Journal" for November 1881 appeared a paper in which while advocating Haeckel's "Physico-Chemical Theory of Disease" the author strongly hinted at the possibility under certain real hygienic conditions for Ronald Ross's and allied developments to arise. And again in the "British Medical Journal" during Sept 1882 appeared a paper which rather more than hints at such an assumption, in which several cases are recorded which afford at least food for reflection.

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* On the Intimate Nature of Zymotic Poison by John Boswell W.B.

** The Etiology of the Fever, Specific Disease by Kenneth Millman B.A. M.R.C.S.
To me it seems that in the doctrine of "Development" and "Origin of Species" he has the clue to much that is mysterious. Given life if the conditions which determine a certain line of development persist at different times, why should not the same line of development recur. It is due to a specific form, as there is every reason to believe it is, may surely be developed again. If the same sort of conditions persist, that were efficient in the origin of the Sealed From Germ. Formulated Briefly my contention is this:

Most diseases with which we are acquainted have their origin in tangible and living forms. These organisms are low in the scale of life, and have an extremely short life-history. A germ produced from a pre-existing germ at this moment, may before many forms have passed, have given
Rise to other, which again in their turn in an equally short span of time, may give rise to further more successive crops—e.g., if into a rabbit a small quantity of septiemic fluid be injected, it may experimentally be shown that in a few days time the blood will be teeming with thousands of minute organisms which go on multiplying till they exhaust the source of food supply which are necessary for the maintenance of life, in the various living cells that go to make up the various organs of the rabbit— thus death of the whole animal ensues.

Being few organisms and having a short life history many generation of germs may succeed each other rapidly, and a consequent possibility follows that surrounding circumstances may modify and appreciably the constitution.

And organization of these forms, so that it is possible for a germ harnessed to become extremely virulent—

That this is no mere speculation but actual fact—Pasteur has demonstrated experimentally. The results are too well known to render it necessary for me to do more than briefly indicate what some of them prove. In his experiments on "African Fever," in the first place, he has conclusively proved the possibility of completely changing the character of a given form, by successive cultivation. Starting with a virulent form the obtained after a given number of cultivations in a sterile fluid a changed organism, unmistakably derived from the harmful parent, but quite incapable of exercising its functions, in other words quite incapable of causing the same virulent type of disease that the parent form could. Indeed by continued cultivation...
In his endeavors in demonstrating that a point of absolute innocuousness can be reached, we actually in an artificial laboratory it was then proved to create changes in an organism sufficiently tangible to be appreciable by ordinary observation. It at least then this fact is proved that the forms we deal with in disease are well-fitted in character and that if they could be experimentally modified then in natural laboratories existing around us for more quittance modification may be possible. Not only has Pasteur demonstrated the fact of changeability, but also other workers among them, some more remarkably than the Rev. D. Halligan, an experimentalist and a patient investigator, who has been superior in the division of biology which relates to the origin and development of the common forms of animal and vegetable.
Vegetable life — In a comparative recent address in his capacity as President (for the forty-seventh) of the Royal Microscopical Society, Dr. Dallinger, for an account of a research (his magnum opus) which has required something on the part of unremitting and careful observation. The research is interesting, and bears so directly upon my subject matter that I shall hope to regard as justified in alluding fully to it.

All acquaintance with Dallinger's work know that he has long ago expressed the opinion that if we would learn how animals accommodate themselves to changed environment — to produce what we call new species, we must study the minute forms of life with unremitting attention. It is obvious that temperature must be a potent factor in the life history of any organism, especially perhaps in those which
Which are little more than a cell containing Protoplasm. Already familiar with
the life history of the Monads Dr.
Ballinga determined to watch their
behaviour under gradual increments of
temperature. Although a new generation
comes into existence every four minutes,
or so, it took years of observation to
raise the temperature to the rather high
point of 158°F. The apparatus employed
was kept in a room at a fixed temperature
and the water in which the organism
existed was in constant communication
with the microscope stage, so that
the minute forms of life could be
observed at any minute. The
temperature of the surroundings could be
raised by degree or half degree, the
thermostat working controlling the
variation to 1/6 degree F. in 24 hours.

Commencing at the normal
temperature of 60°F four months were
occupied in raising it to 70° without—
However, affecting the Monads, which still went on with their multiplication by fission as before — when a temperature of 73° was reached, however, an adverse influence seemed to be exerted on the organism as regards their vitality and productiveness; but by keeping the temperature constant for two months, the new generations became acclimated to a temperature of 78°. Here again a long pause became necessary, while the process of adaptation went on. During the interval a marked development of vacuoles was observed, on the disappearance of which it was again possible to raise the temperature. Long pauses, vacuolation, slow advance were the three stages well marked in the life history of the Monads. Then experimental until the high temperature (for Monads) was reached of 158°F, at which point the experiment was
terminated by an accident. Although
the premature ending of the investigation
was unfortunate, the work done remains.
Darwin is enabled to tell the world
that by a gradual changing of the temperature
he found that organisms which
flourished at 65° were killed at
140°, were in the course of half a
million generations capable of flourishing
at a temperature of 158° and were
killed by a return to their original
temperature of 60°. Here then we have
a proof that organisms can adapt
themselves to changes of a decided
living character, and this evidence
rendering support to the evolution theory,
the few antecedents applicable to low forms of life are
the fields for speculation opened up
by such works as Pasteur's. Darwin
are as wide as they are beautiful, and
I do not pay too much I think when I
declare that they usher in a new epoch in
the history and practice of medicine.
In diagram development we deal with organisms of a low order demonstrated by
Darwin and Challenger to be at least changeable in type. May we not go one step further and speculate also as to their changeability of species? Up to this I am perfectly aware the question has been but little entertained, it is just one of those speculations which do not admit of proof.

Because it has not been demonstrated to prove that higher organisms have ever changed their species, the conclusion of those is pushed at that such a thing as justification of species is absurd.

DE Thetford, in an admirable book on the Human Species published five years ago, devoted 200 pages to an attempt to prove the nonscence of such an assumption, though expressing the greatest admiration for Darwin, he criticise the assumption of the "origin of species" in terms of the
Greatly to Darwin, his theory being almost entirely based on the assumption that no fertile hybrid can ever result from the crossing of plants or animals of different species. I have however taken the trouble to look into the matter, and I have the good fortune to be able to quote from papers which appeared in the "Botanische Zeitung" and in the "Naturforscher" by Dr. W. O. Forke, observations which at least do not render the fact quite so certain as De Zateufel's would make one believe. Forke studied particularly the behaviour of different species of "blackberry" existing in Europe. Since 1857 i.e. prior to the publication of Darwin's great work, he has devoted study to this group of plants. This is what he says: 

"That the blackberries do in fact very frequently produce hybrids is certain."

"Rubus Coesius", a well-known variety of the plant, fertilized all other species with which it occurs in common. This was often accompanied by its Hybrid Boggy. It has often been doubted whether permanent species can arise from hybrids. "Hybrids between species mutually cross from each other are often sparingly fruitful. But we find, e.g., in "Rubus Coesius" and R. dumetorum (two well-known species of blackberry) in favorable localities, all intermediate links between sterile and fairly fruitful specimens.

The original lack of permanence in hybrids is a numerous tradition. Prof. Focke further adds: "If we consider that the majority of cultivated plants have been produced by crossing, whilst all our arts, all our exaggerated influence of soil and climate have not been able to effect..."
much change in given natural species, we shall not be able to resist the
conviction that the crossing of species
process has a great effect on the
formation of new species, than has hitherto been credited.

I have quoted the above to show
that the whole question of "Origin of Species"
is still decided on debatable ground.
That being so, surely the Zoologist
and other working on the line have fallen
into error, when instead of beginning
the study with the lowest of organisms,
they go to the other extreme of study the
"highest" for proof of their assumptions.
It would be manifestly rash
to jump to a positive conclusion
with regard to at present an insufficiently
proved theory, the origin of one species
from another, but this I do submit
that in a study of disease phenomena,
or in a close observation of the behavior
of the low organisms associated with them,
then lie a possibility of a solution of this difficult question which is not fully appreciated, and the possibility rests with students of the conditions of life of these low organisms, in other words with students of "Modern Pathology" to supply the links wanting in the admirable chain woven by Darwin & other great workers on his lines—

In this lies the value of the work. In this lies the posterior addition which it may or may not be possible for students of modern pathology to make to a very important department of human knowledge. Compare for a moment—how favorably situated our "disease germs" are for study in this connection, as opposed to higher organisms. Take man—an organism made up of infinitely numerous parts—every organ composed of an infinite series of living cells—Consider what must happen before a change even of type is possible.
possible, to say nothing of a change which will permanently perpetuate itself in the offspring. Why infinite generations would be required before a type differentiated from the parent could become permanent and perpetuate itself, and infinite generations would comprise for a man a number of years not easily measurable by ordinary human experience or calculation. In one "ancestral germ" we have on the other hand a "triple organism" for in the scale of life, but even perhaps differentiated but even as highly as the individual "cell" in any one human organ. Not only is this organism slow in the scale of life, but it has a power of multiplication which renders infinite generations possible in an extremely short time - a few days suffice to supply us in this connection with a much scope for observation, as thousands of
Of years in connection with higher organisms. Speaking for myself I may justifiably say that I have closely watched such diverse phenomena as have come under my notice, and my observation have only strengthened my suspicion as to the variability of forms. Written records which I have watched equally closely, are further testimony in support of this possibility. Briefly, let me refer to a few of many observations made by me, and to a few published records which seem to me to bear on this matter. Not long ago an epidemic of pneumonia occurred in a neighborhood where I was practicing. It is scarcely necessary for me to go into modern ideas with regard to this disease, but I may as well say that I am entirely agreed with those pathologists who regard it not as a
disease of the lung proper, but—as a fever having for its distinctive character certain changes in the lungs, just as in the same way small pox is a fever which has for its distinctive character certain changes in the skin.* At the same time with this epidemic occurred an Epidemic of *Rheumatic Fever,* and an Epidemic of *Recurrent Fever.* (By recurrent fever I mean the disease spoken of in any next chapter on *Kumbale Fever,* it is the same as the Belgian *Malaria Fever of India.*—Now Recurrent Fever or we all recognize as a *Malaria Fever.*

* In support of this view, indeed in proof of this view, I am able to say that ordinary *Cronin's Pneumonia* of man is in all respects identical to the ordinary Epidemic Pneumonia of cattle. I have in my possession a lung I removed from an ox which died of this disease—

This indicates, in a beautiful manner the pathological identity of the disease in question. The naked eye shows—

° Congestion, red + grey Kepatisation
...mentation are well marked, & micro-

Scopically, the characteristic pathological

Changes are in all respects similar —
In all the air cells, some, I believe, with fibrin are found. The disease
in cattle as in man. Aston often
by crisis — It is as is well known

Intensely Contagious, which leads to

Cattle sometimes. Succumbing — Usually
with effused pleural fluid is practiced
constantly to prevent the spread of
the disease, so that we are brought
face to face with the curious & interesting

Phenomenon that Cattle & man seem
to be peculiarly liable to some common
diseases, which are curiously related
to each other. In small pox — we have
an intensely infectious form affecting
man, but scrofula preventable
by inoculation vaccination, etc.
Infection in the form of cow pox
in which form it affects cattle. In
Pneumonia a form which when
it affects man in not extremely infective, transferred to the ox, intensely infectious, not preventible by inoculation —.

The subject is intensely interesting and tempts me almost to widen depression what I think regarding it — however, that what I believe myself to be in a position to prove regarding it, I must perforce leave to some other time.

For the present, I merely mention the "new theory, in support of the true Theory of Pneumonia —"
A malarian fever — Rheumatic Fever.

Macleay — one of our first authorities on the subject, and the collaborator of the Gallician treatises — the disease declare to be also malarian, in origin. If we agree with him, and I for one do agree with him, then the Remittent, Rheumatic fever must have more or less allied conditions causing them, and be subject more or less to the same causal laws.

Now in one house a Man was attacked with Pneumonia, his wife with Remittent Fever — both fell ill at the same time, both had typical attacks. The wife suffered from congestive chest complications of a decided kind during part of her illness, a significant fact when taken in connection with the pneumonia of the husband. Here comes off after a while. In another house, one child developed Pneumonia, another
Another Child Rheumatic Fever. Both fell ill at the same time both had typical attacks. The thought suggested itself very forcibly that the simultaneous occurrence of a pneumonia in one house with the malarias, recurrent fever in another house with the malarias, Rheumatic Fever was an indication that in some way or other there was an associated causal condition for the three different diseases.

At Kimberley (The headquarter of the Diamond Mining Industry in South Africa where malaria prevails) both by my own observation, and by that of others it is demonstrable that the natives engaged in the mine invariably develop Pneumonia after a heavy fall of rain—how then Europeans who as a rule are subject attacked by the ordinary Malarias. Fever of the place develop Pneumonia in epidemic form at the time.
* The full statistics for the months mentioned are as follows:

- In May, 69 deaths.
- In June, 65 deaths.
- In July, 96 deaths.
- In August, 107 deaths.

Total: 337 deaths.

Of the 337 deaths, 101 represented Europeans, 203 represented Kafirs, and 33 represented other colored persons.

273 were males,
64 were females,
267 were adults (over 16 years),
170 were children.
Mortality statistics for the months of May, June, July, and August 1887 which I happen to have by me at this moment - I find that the deaths from Pneumonia amounted to 87 from Remittent Fever 29.

Another year Remittent Fever heads the list.

Now, taking all these things in conjunction, & considering moreover that Nature's almost everywhere are not - strongly susceptible to ordinary Malaria, the thought definitely occurs that here under circumstances unfavorable in the nature to the development of an ordinary malarious fever, conditions which are known to be favorable for the development of malarious germ, become favorable for the production of Pneumonia. In addition the evidence derived from the coincidence (often of the substitution of the one for the other) of Pneumonia & Remittent
Even at Kimberley, seem strongly to add strength to the view that an identity of causal relationship exists for some forms of Pneumonia, rheumatic fever, etc., and that this relationship exists, but if so, it is probable, if pneumonia, rheumatic fever, and remittent fever are as we have every reason to believe, are forms of diseases, then the speculation to a possibility of some transmission comes to be at least entertainable.

In other words, the conclusion is suggested that forms bred under related conditions, finding dissimilar circumstances in different subjects, might be assumed to possess the potential power to develop in different directions, and cause in different individuals different results.

This will probably be regarded as a wild speculation, and so perhaps it is. Facts, however, are few, and from those I have above adduced.
...other. I hope further on to bring forward any deduction in its render possible. At least it will be conceded, there may be truth in it...true a very different realization of the relationship existing between disease phenomena different regarded as having no relationship will be opened up...I maintain that with advantages of this sparsely populated for the kind of clinical study questions of this sort are of a kind which ought to come up. The subject is beside so full of practical bearing that it acquire additional importance. Is for one of many that instance that suggest themselves of the kind of practical application possible, take the case of any ordinary acute fever— In the course of a pneumonia perhaps a thyroid condition supervene or in the course of some ordinary fever a
Pneumonia suddenly develops, & carries the patient off. How perfectly explainable this becomes when we assume the possibility of a change in the organization of the germ which constituted the original infecting factor. For various reasons the "Pathogen" in the body at the time of infection prepared by an individual germ may become modified, or may become exhausted - one of two results must follow - either the germ must die or develop an aptitude for changed circumstances of life. This is so low an organism as Pasteur has shown these will occur without some change in its character, a change in character creates -he has shown modifications result.

Let us however bear in mind speculation alone for a moment and quote such further facts as occur to me bearing upon the subject.
In the Lancet, Mar. 18th, 1880 a case is recorded by Dr. Kenneth Milligan, which has a startling bearing upon what I have just written. I quote his words as far as I can amicably do so. He wrote:

"I first saw my patient, a strong healthy farm labourer, 20 years of age, on the third day of a severe feverish attack, when I found the evening temperature 102° F.

He had been out of work some week or ten days previously. He lay prostrate all the time delirious, and had two or three 'bubblie-like' looking patches on the face. It proceeded with difficulty in detaching a small portion of the membrane from the face, but found on my next visit that the patch had been renewed. On the 12th day of the fever a crop of "rose spots" were visible on the body. These both in their character and appearance resembled a rash of Syphilis. The temperature had risen
mean to 105°F. * * * On the 15th day, three days after this extreme temperature it fell again to 100°F. The patient seemed less ill. The mother called hay attention to some pimply papules on the body.

"The Seventeenth day - 2 days later - saw the first crop of rose spots almost entirely displaced by new ones. By the 21st day of the fever, or six days after the appearance of the papules, he looked as the mother remarked, for all the world as if he had been vaccinated.

"The papules ran the course of an ordinary small pox eruption, and after the 30th day of the fever he began to mend rapidly," Dr. Milliken added.

"The combination of symptoms here was remarkable. But—far the astonishing nature of the assertion one would have felt inclined to say that the patient was suffering from a combination of Typhus Fever
fever, with Diphtheria and Smallpox. It was found quite impossible after the most careful search to trace any part of contamination from any one of the diseases which were here presented or counterfeited. There was no possible chance of ordinary infection, for the patient lived in an isolated village, from which he had not been so far as four miles for many months. "There had been no contact with any person labouring under any kind of infectious disease. There were no cases of infectious disease of any kind present at the time in the village or neighbourhood."

It will be readily admitted that the above case, if the record be faithful (and from the care with which the record is made we have no reason to doubt its truth), it will be readily admitted offer ample food
for reflection, and suggests strongly that by some subtle influence existing at the time in the body there was a struggle for development of the infecting factor. Either this must be inferred, or the conclusion arrived at of infection simultaneously and separately by the specific forms associated with the diseases named—none of which diseases in the closest manner possible. Dr. Hillian asserts were anywhere to be found at the time. Again take the following record. In the British Medical Journal Feb. 1, 1879, Dr. Holland records a case where a pupil at a school contracted "decided scarlatina" from two new pupils who "though doing ordinary school duties, had a nasal discharge & excoriation of the lips attributed to cold." During the second week the pupils fell ill in rapid
Succession to the number of 24 or 25, and the school was broken up. The fowl sickness presented—and this is the remarkable point—every conceivable variety of diphtheria and scarlet fever. In other words from an apparently anomalous indeterminate infecting source, common to both, "seemed to arise two diseases hitherto regarded as specifically distinct from each other." The explanation suggests itself here again that an indeterminate germ, originally finding dissimilar conditions in the various pupils, developed in some in the direction of scarlet fever, in others, in the direction of diphtheria. Curiously coincident with the explanation of these cases are some remarks made by Professor Hackett of Graysville at the International Medical Congress at London in 1887—He spoke as follows (vide Transactions Int. Med. Congr...
Practically the question of the specific functional activity of microorganisms and their relative unity may be stated in this way. Is it necessary that a person who falls ill with scarlatina, measles, or smallpox, must have received the infection from another individual affected by scarlatina, measles, or smallpox? "I say in my heart: answer the question with No! While the defender of the theory of absolute specificity must answer it— in the affirmative. I consider it possible that at any rate— the pathogenetic processes may take on such a course that microorganisms arise from them which produce scarlatina. The disease might then originate at this spot, without there having been any absolute continuity of infection from another case of scarlet fever.
Their words coming from an observer like there are at least entitled to consideration, and if the cases I have quoted above are worth anything, they simplify in a marked manner the truth of his assertions.

But let me see whether in the phenomena observed in a country like this there are not also other facts bearing on the question. And leading apparently colour to the theory of variability of gene.

For a very considerable period now medical opinion has been divided at the Cape as to the true nature of some severe fevers. As I have already said above we have frequently cases of true Malarion Perpetual Fever. It goes without saying that here as in all communities we have also many cases of True Enteric Fever. In districts, however, where many of the oldest and most experienced
* The Kivu occurs of course also in localities where "Malairia" exists, and where the febrile fever is found.

For my purpose at present, I mention it in connection with localities where neither are supposed to exist, in order to emphasize its occurrence, as separate and distinct from both.
Medical men declare Malaria in the ordinary sense to be absolutely nonexistent; and where true Typhoid Fever is also extremely rare if at all met with, there occurs a fever which for want of a better term many medical men call Typho-Malarial*. Yet, there is extreme doubt in their minds as to the truth of the "Absolute Specificity" doctrine, as amply indicated by the fact long ago the subject was regarded as of sufficient importance for an exhaustive discussion at one local medical society, and the conclusion arrived at was distinctly in favour of the existence of a fever which could not be classed. The mere fact of the existence of such a doubt seems to me to justify the speculation I have entertained. That it exists in the medical mind here, & elsewhere also the pages of our medical journals daily show —
In the British Medical Journal for May 16, 1889, appeared a thoughtful, elaborate, and well-considered paper on the British Medical Journal by Dr. David Bruce, assistant Professor of Pathology, Royal Victoria Hospital, Netley, on the subject of Quincke Fever. He defines this fever as an endemic disease of long duration, characterized by fever, continuous, remittent, and intermittent, in type, in most cases, with enlarged spleen, profuse perspiration, abdominal constipation, relapses almost invariably, accompanied by pains of a rheumatic or neuralgic character. Sometimes, swelling of joints or Testicles, leading almost always to recovery. In fatal cases, enlargement of spleen, congestion of the mesentery, and upper part of jejunum, no swelling of Peyer's glands, or ulceration of them, at the constant occurrence in various organs of a species of micrococci.

After discussing exhaustively its etiology, its mode of prevalence,
a conclusion which demonstrates what the unconditional acceptance of the absolute specificity doctrine leads to
its relationship to enteric fever and treatment. He arrives at the following conclusion (in which he demonstrates what the absolute acceptance of the absolute specificity doctrine leads to): "Malaria fever is a specific disease quite distinct from enteric or \textit{Recurrent} fever."

Now from the description given, I do not think there is the least doubt that this disease, what many call here \textit{Typho-Malarial}, is I am firmly convinced that while some cases, more or less, represent departures from the specific type of true enteric fever, other, or probably the majority are nothing more than ordinary \textit{Malarious, Recurrent} fever, in which the exciting factor has been changed by environment to sufficiently to create a modification in type of the disease. I am quite convinced that the form here sketched is to an almost absolute extent nothing more or less than the \textit{Malarious} fever. I allude to in my next paper to \textit{Kumhali} fever.
Not long ago a paper appeared in the British Medical Journal by Mr. Macartney, Surgeon-Major, Medical Staff, India, in which he says:

The author quotes: “There are none who go to the length of naming it typhic malaria, or again who even when ulceration of Pepe’s patches is found on postmortem examination, still believe from the peculiar course of the fever that the disease is of the Chronic Recurrent Type.” In proof of the existence of the latter class, I extract the following from a recent member of the Lancet:

The Bowel Lesion in Typhoid Fever.

“Now, the question of the intestinal ulceration met with in typhoid fever to be considered pathognomonic? In the detection of ulcers in Pepe’s patches, presenting bone of the character of tuberculin, or dependently, to be taken as a proof of typhoid fever.
"Even in the absence of well marked clinical symptoms? Are we justified in believing in latent typhoid from post mortem evidence alone? Such are the questions which are suggested by an interesting brochure recently published by Dr. Vandyke Carter upon "A Peri-jejunal Ulceration of the Small Intestines apparently new (Bombay 1884), and the answer he would impel us to give to them would be a decided negative. The material upon which he bases his paper consists of about ten cases in nature (in whom typhoid seems to be rare) suffering from symptoms described as remittent fever, and after death, which in some cases was due to perforation peritonitis, exhibiting more or less extensive ulceration of the small intestine. Constipation rather than diarrhoea was the rule, and in fact more
more than one case did the fever run the course of typhoid fever. But Dr. Vandyke Carter does not rely on clinical grounds to establish the non-identity between the intestinal lesions in these cases and that present in typhoid fever. He subject the type of ulceration to a close scrutiny, and points out several prominent features of difference, the most striking being the absence in his cases of the excessive swelling and infiltration of the Peyer patches that precede the stage of ulceration in typhoid fever. He inclines to the belief that these non-specific ulcers are produced by embolic or thrombotic plugging, and that this depends on septic conditions. That septic causes are adequate to produce intestinal lesions of this kind is not to be gainsaid, and
and we think that Dr. Carter has made out a case for further investigation. A less cautious observer than Dr. Vandyke Carter would doubtless have been satisfied with the detection of the numerous bacilli in the favored seat of infection, & the occurrence in many cases of perforation, to declare that the cases came under the head of Typhoid Fever, modified it may be by the conditions under which the disease occurred. Attraets we have been led to rely upon the elevation of the intestines as the only positive criterion of this disease, but now it will be necessary to review our position in regard to it & it may be that we shall have to fall back upon the detection of the typhoid bacillus to determine the diagnosis.
In old crowded countries it would be
difficult to point to any spot where
at one time or other discomfort from may
not be learned to have existed —
The etiology of typhoid is best to be borne in mind is by any way of thinking not by any means so well established as most European observers would have us believe. The idea that typhoid must be propagated from the defects of persons affected previously has been demonstrable in countries like England. Really thought it will however ill explain the occurrence of typhoid in countries like this where now & then in insanitary condition the fever arise. Suddenly where ill only the typhoid was before expected but where no human body was ever aggregated. Thus during one of our late native wars, it was not uncommon experience for strong able-bodied soldiers, camped in the open field, to develop under the insanitary conditions forced upon them what competent observers have classified as true typhoid.

*The Zulu War.*
To write such attacks to reference to the defects of previously affected person would be as unreasonable, as to deny a cause, associated with the natural septic after-effect processes, which would naturally go on under such circumstances.

To suppose that in an isolated part of Africa dormant or non-capable of breeding Typhoid fever, and only Typhoid fever should for countless ages have been lying in wait for an opportunity to develop seems to me as unreasonable as to deny, under the circumstances known, the possibility, for innocuous serum floating above to become specific properties. Surely if Pasteur can modify his serum as he undoubtedly has shown as he can, if Dullinger can create an organism capable of living under conditions which would be impossible for its parent to live in, we must be constantly in nature, having a condition of this
interest for these low organisms also, and possibilities for change which it would be inexcusable ever to be able to simulate in any laboratory. And if that possibility for change exists, is there any difficulty in explaining the phenomena? All who have practiced in countries like this, have had frequent opportunities of observing, in connection with such fevers as Typhoid  
Remittent, so called Typhus marial fever, granted a capacity for a given time to evolve in one or other direction, it is surely not difficult to see how possibilities will arise from more or less identical causal conditions, for different, more or less allied types of disease may arise, but types however absolutely distinct from each other sufficiently so as to be recognizably as separate entities... to deny the fact— that clinical observers all the world over, are more or less compelled,
(If they intelligently observe the phenomena they are daily brought into contact with), to question many of the conclusions of the absolute specificity doctrine, would be to deny, the evidence of records from all sides, published every day, would be to ignore, the constant difficulties with which every day one is brought into contact, with certainty, to the admission of the absolute inability to find any reasonable design in many things which I for one prefer to regard as the result of some rational design.

Writing in 1872, Professor Stokes (vide British Med. Journal April 13th 1872) in a discourse on state medicine says, "No one who has not had a lifelong experience of epidemics, can estimate the difficulties which start as to their origin. . . . xxxxx. 'The appearance of epidemics at irregular periods, while they are supposed to indicate causes remain
remain constant. Their disappearance through the causes continue in full operation; their outbreaks in all latitude, climes, and seasons; their different modes of spreading; the want of constancy in their symptoms and history (for every great epidemic has its own character); the variety as to the extent, nature and effect of the secondary affections which arise in their course; the variety of the mode of subsidence, and behaviour under treatment; their degree of mortality and contagiousness; all these things constitute the difficulties which surround us in our investigation as to putrid diseases. They bear on the supposed specific or constant origin of disease, on the error of drawing hard and fast lines between essential affections and are with difficulty reconcilable with the Germ Theory. Reconcilable with difficulty, indeed,
Indeed I would add, if we regard with Professor Stokes on disease germs as independent and fixed entities. Reconcilable with the greatest ease, if we would once allow our doubts and perplexities to be illuminated by the light of ‘evolution’ in as far as it exercises a modifying influence on our disease germs. Illuminated by this light Professor Stokes, difficulties not only disappear but so perfect a resolution of all perplexity is suggested, that one is irresistibly compelled to fly to it, in order soundly, practically & philosophically to clear up doubt, & an irresistible tempted to accept its application in association with the Germ Theory as one of the grand facts of Pathology.

So far I have been concerned in my remarks only with the
Exogenous agencies in disease, with the
factors associated with disease, with the
exciting factors of disease.

I have as yet left untouched all
consideration of the internal mechanism,
that mechanism contained within
ourself, by which resisting power to,
our susceptibility to the exciting factors
is determined. As far I have considered
only seeds, a word or two on the subject
of soil may not be out of place.

This question of soil is an element
not to be disregarded, it associated
with its adaptation to various directions
are the question of pathogenesis in
disease, the establishment of weather
in climates. Immunization from disease

The living body it must be
remembered is built up of living
cells, elements. These individually
must as well as collectively obey the
same laws that living matter elsewhere
does, and, so far as they are concerned it may be assumed that a constant adaptation to surrounding circumstances goes on. A simple illustration of this fact may be advanced. I have by me at this moment the official returns of the annual mortality per thousand inhabitants at Sierra Leone from 1829 to 1836. From these it appears that while 150.2 per 1000 Europeans annually die from measles fever, only 2.4 Negroes succumb. This will amply demonstrate what I mean. On no assumption can the disproportion in mortality be explained, but that by which the Negroes are credited with a special resistance to venereal infection. This sense of resistance can only have resulted from a partial adaptation to their surroundings, if the living cells in successive generations of Negroes, an adaptation which in
in transmission from Fatten to son
become intensified sufficiently to
create ultimately an almost complete
immunity from smallpox.

On the assumption of such adaptions
can also be explained the occurrence
of acute fevers in epidemic form
such as e.g. smallpox etc.

It seems to be reasonable to
assume that when a series of
conditions climatic or otherwise,
come into operation to favour
the development of germs outside
in certain directions, that the same
laws acting upon the human
organism may create for the time
an adaptation of living cell elements
in that organism, which for the time
may render them more susceptible
to being influenced by these germs.

When these conditions alter, these
retrogressive development may equally
be assumed to go on in form outside
to a point of uncommon measure, in incapacity to infect, as well as a return to normal existing power of cell element within the body.

If this were not so, it would seem to me we are to say to explain the rapid disappearance of epidemics. We know that in a disease such as Small Pox or other acute forms, every person attacked multiplies in a very enormous ratio the quantity of poison infecting material. Then therefore an epidemic is at its height, the quantity of infecting material must also be at its maximum. Instead of going on however, we find even in the most populated communities, where there are always large numbers who are still be attacked, that the epidemic dies out not only, but also remains away perhaps for a cycle of years, and then appears...
Again. It no retrospective development as that incident occurred it would be difficult to explain this—The year after the epidemic of small pox of 1887 which as I mentioned in the beginning of any matter, raged severely in Capetown at the time, an epidemic of equal severity raged at Kimberley 300 miles away—There being railway connection between these two places, many cases from Kimberley were brought—down unknown whilst the disease was incubating, & the attack broke out in ordinary lodging houses etc. after arrival in Capetown.

There was however at the first spread of any kind. Either it must be assumed that these cases came into contact with Ione who could catch small pox, an assumption found entertainable, or the development.
Development of man or less.

Special resistance for the time
in the community at Cape Town
be accepted.

Dr. Bondin in an interesting work on
Osteological Pathology wrote as follows.

"Elephantiasis, that affection by
which certain parts of the body are
sometimes deformed in so strange
a manner as found in the Indies
and at Barbados."

In the latter islands negroes alone
were attacked by this hideous disease
till 1704. One white was in that
year affected by it— for the first
time. But the disease made way
so in 1760 it had extended to the
Creole population. Whites of
European origin have as yet escaped.

"The Elephantiasis of India is
found in Ceylon. There again it
attacks only natives, Creoles, and
individuals of mixed blood."
Europeans are exempt — Only one case of this had been observed in a European white, but this individual had inhabited the island for thirty years. Acclimatisation he add had been carried so far in this case as to cause him to lose his ethnological integrality. In other words, the acclimatisation has some way or other must have modified the internal mechanism of this European sufficiently to create a susceptibility to disease for him, which before did not exist. Only on this assumption can this fact be explained.

In the Lancet of December 18...

1892 appeared a remarkable lecture by Sir James Park on "Some New Treatises," in which a paragraph occurs so admirably explaining what I would like to express that...
I may be pardoned for alluding to it. This is the passage. Perhaps the brilliant successes which have been achieved by the recent studies of disease producing organisms & of the materials acting on us from without - as success not equalled in any other field of medical enquiry - I have made some think too little of those changes within ourselves which occur in such ordinary condition of life, that they may be called spontaneous.

Yet these are not less important in the production of disease, & these must be studied, just as in agriculture soil must be studied as well as seeds. This is true even in respect of those diseases whose essential causes are most evidently external, even of those which are due to specific contagion.
fertilize in an unfit soil.
I suppose there is but a day in which most of us do not inhale or come into contact with the germ of some frequent or contagious disease, but they do not fertilize in us, any more than do the seeds of tropical flowers in our streets, or in the fields to which the wind scatter them; we do not offer the fitting soil. And even among those in whom they do fertilize the product varies according to the soil. And the study of this soil, the living soil is yet more necessary in respect of disease, which come in part or wholly by inheritance, for it is in each as personal or distinct as any other constituent of personal character, and the study of it must be ultimately personal.

The element of soil then is an element not to be disregarded. It would be interesting to pursue the
To enter into a discussion of these points here would lead me to a digression quite beyond the limits I have set myself. I mention them as bearing on one phase of the subject. Elsewhere (S. A. Philo. Soc. Transactions 1885) I have touched upon them somewhat more fully.
Subject further. The consideration for example of the curious fact connected with the immunity given by one attack of some disease, against other attacks of the same disease, the stable hereditary transmission of disease, of Hereditary transmission of disease & c. All these are matters which concern the question of soil rather than breed. For the present, however, I must content myself with what I have said. May I venture to hope that I have in a small measure succeeded in demonstrating that it is not altogether an unimportant for us to recognise in Pathology, & at the bedside, as in other departments of science, the great fundamental principles indicated by the Law of Evolution, & that on the assumption of its applicability to questions of disease what we regard as abnormal
Process, may be brought into normality & unity with every thing around them.

It remains for me but to summarise my conclusion:

1. In the small pox outbreaks described above, vaccination prevented the disease in all who had been previously vaccinated, when resorted to early enough.

2. It modified the disease in all, where infection had already taken place at the time of vaccination.

3. It was efficacious in preventing the disease even when performed after intimate contact of un-vaccinated persons with small pox had taken place.

4. In all cases where un-vaccinated persons (during the epidemic) were exposed to infection, the disease followed, mostly death also.

5. Vaccination gave an absolute immunity against attack of small-pox.
6. A predisposition to smallpox existed at the time of the epidemic, as indicated by the vaccination results.

7. There is a possibility that smallpox may arise without continuity of infection.

8. As with smallpox so with other germ diseases.

9. This is difficult of proof, but in the thinly populated condition of S. Africa, proof or disproof may be found if we carefully supply systematic records of our observations.

10. All living matter is wherever found subject to the laws which of evolution.

11. Pasteur and Darringham have in their laboratories demonstrated the want of fixity in the germ of disease, and change at will the type of organism experimented upon.

12. It is a question for argument whether slow organisms may not...
It is impossible to lay down hard and fast rules as to the variability in different directions or forms. Absolute scientific proof of the difficulty, may be impossible, but here too, in sparsely populated countries, proof or disproof may be found, if we carefully note down our experiences.

Not only germ outside, but also the living cell elements within the body are subject to variation from time to time.

In this way, special susceptibilities or resisting power to disease arise, the same law which favors the development of forms outside to a point of infecting capacity, favoring the development of the cell elements within the body to a
15. Environment determines type in all living organisms. Even disregarding
the speculative possibility of a
change of species in germs, it
is demonstrable that departures
from specific types of disease, exist,
which are caused by departures
from specific types of the germs
associated with such disease.

16. What are often regarded as
abnormal processes, are only
additional evidence of the
uniform influence of the
laws that regulate all things
alike.

J. H. Henry Beach
III

An Enquiry into the Cause of the Camp Fever of Kimberley—S. Africa—
An Inquiry into the Cause of the
Camp Fever of Kimberley.

By all the writers of
Medical double controversy which
have of late years addressed the medical
mind of S. Africa, there are probably
few which have excited as much
attention as that which was known in
the country as Kimberley Fever.
Kimberley, situated as it is in
the islands of S. Africa 600
miles from the coast, is as
is well known, the headquarters
of the Diamond Mining Industry.
A few years ago only, the site
now occupied by a large
city, was unoccupied land;
visited only when necessity
compelled war parties to pass
by it. A few struggling farm
occupied the site of it but little
expected to mark it as a Fief.
There is more interest than hundreds of isolated farms districts in South Africa at present. Suddenly diamonds were discovered, and with the natural result, population flowed towards it. In a few months, hundreds of thousands appeared there in the mad race for wealth. Digging operations were commenced if it was not long before disease made its appearance. In addition to the ordinary ills of life, it was not long before a fever made its appearance. This extremely acute in many cases, carrying off the most valuable of lives. This always been, and is still a serious blow to the development of the place. Its nature for a long time was deceptive, and apart from valuable life sacrificed through this loss —
don-recognition, the Tag Community looked with something more than absurdity upon their medical fellow, who could fail to be 
appalled at the absence of the whence 
and wherefore of the fever — 
Malaria had been unknown 
in this part of S. Africa; Entire 
fever at its deciduous starting shrouded 
it — what was it? 

Shortly after I first visited 
the place, I discussed the 
question with one of the oldest 
and ablest medical men in the 
place, and was told that the 
fever was something entirely 
'new' — unlike anything 
described, both in its inception 
and history. Having the benefit 
of a Toponym in Kimberley 
for many months, during which I had an 
Opportunity of seeing many...
affected, I ended by developing an attack myself. - Immediately after my attack was over, I presented to the Cape Town Philosophical Society my views upon the nature of the Fever, which, while hitherto denied for a time, have now been recognized fairly universally as the property proper explanation of the disease.

As the matter is one of great importance, it was much assistance to me to receive confirmation from many researches subsequently carried out in many parts of the world. From this point I take the opportunity of entering into the subject once more. I am quite sure that the subject is one which has important bearing upon all that tropical disease develop, and my one regret is that I have
Enforced departure from Lincoln after my convalescence, placed it out of my reach to investigate the matter as thoroughly as its importance demands.

The form as it presented itself in myself is entirely a Remittent Type. Low but not severe normal morning temperature, high evening temperature, Obstinate constipation, extremely rapid wasting, most prevalent in Autumn and Spring, the cause it is referable from Affe - The liver is often enlarged, now and then Jaundice accompanies it. It often runs on for months - appear to be uninfluenced by Guinivere or pneumonia as its chief Sequela Rheumatism. Then sudden relapse occurs with all the accompaniments of a full attack. It is a chronic
amongst those who have not previously suffered from malaria, as amongst those who have.

When it first appears, the temperature may be at 102° in the morning and 104° in the evening. As the case progresses, there are peculiar upward flutters of temperature, followed often by corresponding depression, and later on in the disease periods of subnormal temperature followed by sudden exacerbations. The spleen is always involved, but in this point many of my colleagues differ from me. Personally, I have no doubt of it. Now and then apparently complicated, an attack —

Viewed by such a description of symptoms there are manifest many points of similarity between the Fever of the ordinary Malaria from of India —
Kummerly's nervous was a place not offered to brush or the mind of moisture of India. Vegetation was non-existent, and if malaria, there were clearly conditions here causing malaria as development which had been or if so, but shortly recognized. That the fever was only malaria, in origin, I had but little doubt after my attack. The explanation of this term however for a long time remained a mystery. Gradually, however, matters became clear to me, and I reflected upon the condition. After my illness, I felt myself able to supply an explanation which in my mind is theory no longer, but fact, carrying the clearest conviction with it.

Most minds associate the development of malaria with spurious places containing organic
Matters such as, e.g., decaying vegetable matter, the total absence of undue moisture from Kimberley as well as of excessive organic matter in the shape of vegetable growth. It is doubtless the reason why, for so many years no clear idea on the subject has been arrived at. The fallacy must be dismissed, indeed recent researches have demonstrated once and for all that for the development of malaria. detta drench moisture or much organic matter is an absolute necessity.

In support of this fact I am able to quote an admirable paper by Tomasi Crudele, which was reprinted from his official report to the Italian Government in the Practitioners (vol. 27) * To this authority I am indebted for the information*

Of many doubts in coming to any conclusion. This is what he says. I may ask that Klebs unconditionally agree with him.

As we may regard as well proven fact that
1. The Bacillus Malariae is an organism for whose growth the air is necessary.
2. The form a formula of this organism may occur in soils of very different composition.
Honourable, very few in organic substance, and what is very important.
3. These malarial soils may occur in places which are not never have been malarial.

As far back as 1848, I find a note on this subject in the lecture by Sir J. Murray, who in the absence of quinine avoided malarial fevers by disturbance of electrical equilibrium in and outside the body. (Lecture Oct. 21, 1848)
In the same paper I find the same authority saying: "In all material soils and mud a development of the Bacillus Malarias into spores from Bacilli, as well as a rapid succession of several generations of these Bacilli occur, whenever they are placed under the following condition:

1. A moderate temperature of about 20°C (68°F)
2. A moderate degree of relative humidity
3. The direct action of the oxygen of the air upon all parts of the mass"

That Malarias should not require for its development the actual presence of decaying vegetable matter is intelligible. I think we may reasonably assume that all soils capable of bearing vegetation, plus air and moisture would contain potentially all the chemical elements which would be required for building up plants.
Plants, and we can hardly believe that in nature it would be necessary for the maintenance of an organism that the chemical elements necessary for its growth should first have to go through the intermediate stage of plant-growth, before it becomes fit for nourishing such an organism. Granted the presence in the soil of ordinary chemical elements required by plants for their elaboration we may assume that in that stage they would under favorable conditions form suitable compounds for the fever associated with malaria. With all this before us then how can we apply them to Kimberley Conditions?

The diamond mines are an integral portion of Kimberley. Out of them come daily thousands of workers of diamoniferous Earth; on reaching the surface their Earth
Earth is carried away and deposited in thin layers on what is known as the depositing floor. These depositing floors extend for miles in all directions, amongst houses, and at the time I left Kimberley, were beginning to encroach on what was supposed to be the healthiest parts of Kimberley. As soon as deposited, every particle of earth is moistened moderately but thoroughly and kept moist for any length of time. This is done in order to encourage pulverisation of the earth, prepare it for the washing machines, through which in due course every particle is driven. Saturated thoroughly with moisture, separated from the contained diamonds, and left as debris known as the "wash up" heaps as they are called. Of these there are
are hundreds dotted all over the place, many of them forming mounds of considerable size. Here they are all the conditions laid down by Romani Cudoli.

The earth taken up is capable of bearing rich vegetation. It contains a quantity of Carboniferous shale, which is frequently used for filling up garden plots, pots for plants. In Australia and the New Country, it has been often noticed that newly ploughed fields have the power of generating malaria. Successive cultivation of these fields has been found largely to destroy this power. Even in England it has been over and over again demonstrated that fresh railway cuttings have caused malaria.

It would appear then that fresh earth possesses a power...
Power of generating malaria, than cultivated earth. The earth near the mines is then not only rich in vegetable potentiality, but is also in the best condition for offering a niche for the growth of plants being absolutely fresh. We may therefore assume that the fresh earth near the mines possesses properties most unfavorable for the production of malarious fever.

Now this earth is spread as I have said on the defecating floors, so here if Tomasi Ndueli had been asked to prescribe a method for cultivating the malarious area, he could scarcely have suggested any means more likely to succeed.

The earth is exposed to the sun's rays, which in Autumn and Spring mean a temperature...
considerably above 68° F. (The temperature quoted above is already sufficient for the development of malaria), we know that in India malarious fever becomes serious
limits, directly more virulent as the places in which they occur near raised temperatures (like Fyren Fwen or India).

6. The earth is on these depositing floors, kept permanently and moderately moist—

7. Being loose or less firmly in process of crumbling, air permeates into it— in all directions. As it becomes so loose through to the oxygen of the air.

8. I am convinced that the cause of the fever is to be looked for here that I believe it—be capable of experimental demonstration.
I had already commenced to institute some experiments, in the hope of being able to verify what I have said above – An attack of fever contracted by myself, or any consequent departure from the place where prevented me from carrying them to any conclusion. Again and again have I urged colleagues or wives besides those to take the matter up, but without avail so far.

I may as well mention the direction I intended burying to take.

I proposed to inoculate with earth from the depositing floors, for a few days after it had been taken from the dune. Several bottles containing a sterilised fluid capable of supporting organic life – such a fluid as e.g. Beef tea sterilised by lengthened boiling. – There
I proposed after inoculation to heat for a time to a temperature of about 100° F. (about the body temperature) in order to allow any organism introduced with the earth to time to multiply. At the end of the time I intended to inject the fluid so obtained under the skin of the a rabbit. I should have expected if my theory were correct, to have produced in the animal an analogous fever. In this test I was anxious to submit my theory, as I have said hereon, the contraction of an attack of fever by myself and my enforced departure from Kimberley in consequence prevented my carrying my intention into effect. In 1883 I first proposed in a paper on the subject that these experiments should be conducted. It is gratifying to find from the researches of
Klebs & Turski: Cradle - (On the Nature of Malaria. Sydenham Society: Translation 1888) undertaken since then that they have successfully applied the practically the same method to the elucidation of the cause of Malaria in the Bantu Negro. Their conclusions on the Nature and Origin of Malaria in every way bear out the view advanced by me.

According to them Malaria exists in forms of very different composition, whilst forms of the same composition, if placed under the same physical conditions sometimes do, & sometimes do not produce it. This shows that its development is not bound up with any special Chemical Composition of the Soil, but is insufficient to explain that other things being equal, its development may be more or less influenced by some Chemical
I see this view expressed also in Shield's work on Malaria. This was published last year (1888) and agrees substantially with what I advanced at the date of my first paper on this subject published in the S.A. Philosophical Society's Transactions for 1883.
Chemical quality of the soil.

All things considered I should regard my view as to the better adaptation of soils rich in elements capable of supporting plant life, to the production of malaria. As most likely to be true, and the latter to be further borne out by the fact that intensive cultivation may modify the chemical composition of a dangerous soil as to diminish its aptitude for generating malaria. It is intelligible that a soil after the chemical elements necessary for elaboration of plant tissues have been abstracted from it, should be in a very different condition than the same soil in a virgin that condition.

In Kimberley the Fresh Earth deposits in thin layers for miles around the place is kept on the surface moderately moist — now accord
to Tomasi Crueldi, up to the
date of his last research (The
Nature of Malaria 1888), the known malaria
haunts which had been up to that
date found to be associated with
malaria production, contained as
a rule a large amount of Subsoil
moisture. This would be against
the theory of advanced by me, to
the amount of Subsoil moisture
at Kimberley, can not be extremely
part. Some of course exist but
not a very great deal. This explain
the non existence of Malaria before
the digging operations were commenced
at all. But Crueldi's research
led him to a very positive conclusion
in another direction. Then in the
work above referred to he says
page 3: "During the hot season
Malaria production in quite the
upper State of the Soil when there
remain sufficiently damp," this
in what we hear at Kimberley.
The upper strata of soil exposed in keep moist and constantly moist and so is most favorably situated for Malaria production. This fact is of very great importance, as it may have important bearing upon means developed for preventing the Fever. It would be obvious then to employ systems of subsoil drainage, when the cause of the Fever is to be looked for in the crust of the Earth, that lower down.

As far I have considered the development of Malaria at Kimberley, where before the washing system, very little under the condition of mining as at present, carried on — before the present system of separating diamonds from the Earth obtained, there undoubtedly existed a very large amount
I am — if I be right in what I have advanced, there should be an explanation also for this. In the early days of the field, the days of dry digging as they are known by, when diamonds were separated from the matrix by processes of dry sifting of powdered earth, there can be the least doubt that the same force as that of today was infrequently met with.

The consideration of this, I must confess caused me for a long while to have serious doubts as to the correctness of my views. But I do believe that I shall be able to offer an explanation of this fact — quite in accord with what I have already said.

And here again let me acknowledge the help I have...
obtained in coming to a satisfactory conclusion from a study of
Klet's and Tomasi Credel's writings.
In Credel's paper already referred to (Malaria of the ancient
Armenia of the Roman hills) he says that he found in the Campagna
of Rome that "a very moderate
degree of moisture was sufficient
to produce malaria. Sometimes
bits which are truly pestilential
may remain unoffensive through
a very hot, dry summer; then
all at once for rise to
an explosion of malaria after
being revived by a 'slight-
shower'. But another fact
he says, "The soil in malaria
districts may remain exposed
to the direct action of the air
during a hot, dry season
without causing any danger,
and may suddenly become dangerous."
After a very slight fall of rain.

In another paper he remarks that "An elevated temperature may provoke an outbreak of malaria in soil which is generally incapable of producing malaria, in such quantities as to give infectious qualities to the local atmosphere, and to demonstrate how apparently slight a cause sometimes may give rise to malaria. He writes as follows: "The custom of keeping a number of plants in heated drawing rooms may become the determining cause of malarial infection, even in localities where malaria is unknown."

This fact has been recorded also by another authority, Prof. Eduard von Eichwald of St. Petersburg.

* On the generation of malaria in flowers.
The mention of a specific case where flower pots filled with dangerous earth, placed in a heated drawing room, were without any kind of doubt demonstrated to have been the determining cause of severe ague in a lady.

Now all these facts are important, inasmuch as with them before us, it is not difficult to understand how fever should have developed at Kimberley during the days of "dry digging." The Rainy Season at Kimberley is in Summer. During the Summer Months, Kimberley is visited by Thunderstorms of terrific severity. The water pours from the heavens often at a rate which is quite alarming. This being so, there is nothing remarkable in the
or incomprehensible in the development of malaria under the then existing conditions.

The whole population in those days was engaged in digging. The fresh earth from the mine as well as the surrounding country must often have been super-saturated by these thunderstorms. It must be borne in mind that the mine was then not very deep, it sloped in all its parts to the sun's rays. With this supersaturation there must have been constantly mounting enough of earth for malaria development, and for the creation of epidemics of considerable magnitude. Moreover if so apparently insufficient a cause as a flower pot in a room can create a, demonstrated by Schwalbe, malaria of an acute type, then in summer the
Fresh earth turned up in the confined tents in which people then almost exclusively lived, moistened by rain, must often have been more than sufficient to induce fever in susceptible subjects. Indeed it seems incomprehensible to me, that there might have been in the early days of the Fields, even more general epidemics than at present. Now a day, many at times have occupations which reduce the risk of infection to a minimum.

In the early days, everybody was a digger, and as such was constantly in the mine, and exposed to it.

All these considerations have practical bearings. If I am correct in what I have said, then clearly correct and definite data will
exist for founding attempts to prevent the periodical invasion of Spheniscus ho. Up to this point, they have remained unexposed to...

For instance, recognizing the conditions I have laid down, as necessary for the development of malaria, preventive measures should be based on methods that might anticipate these conditions. Meaning for example that free exposure of the moist earth to air and sun is one condition under which malaria may develop. Means should be adopted to prevent the air and sun from setting at this earth, where there is strictly necessary. Such means as...
Recognising that proximity to the depoiting floors & wash-up keeps means proximity to infecting centres, some way of running these floors from the populated parts of Kimberley might be worth to.
The direction of winds ought to be studied, & not reduced to a minimum from forms raised from infecting centre by these winds & carried by them to inhabited parts, or now that the Railway has been extended to Kimberley, further suggest develop Suburban residences to be exposed to less danger. But not only as a means of suggesting practicable methods of prevention at Kimberley in a true recognition of the cause of this Fever important. As bearing upon possible outbreak.
I have alluded to this paper (in the essay preceding the present one) as an example of the error which arises in classifying some types of so-called Tylus malariae that are classified as specifically distinct from Remittent Malarial Fever.
in other sub-tropical communities, associated with little known or understood causes, my observations may have value. Not long ago (May 18th 1859) appeared a thoughtful elaborate and able paper, in the British Medical Journal, on the Subject of Malta Fever. The author (Dr. David Byn) apparently not recognizing the true nature of the disease, disputes entirely its identity with either tertian or Remittent Malarious Fever. Do we then seem to have little doubt that the disease in the same as Kimberley Fever? I should be surprised if some cause or and the associated sickness is similar or compound conditions to those sketched above be not actually present and common troubles. Malaria Fever in the Natural
And Civil History of man a higher importance than all other endemic types of disease to which the human race is subject. So speaks Klebs. Human Amebiasis is thisition—any contribution to the proper recognition of its causation must have a wide importance. It is impossible to define the geographical boundaries to the production of malaria. While reaching its highest point in the Tropics, it is found also in Cold Climates. It may render districts uninhabitable. As other endemic or epidemic is able to do this to the same extent. It constitutes the chief obstacle to the exploration and colonization of the African Continent. Threaten to reduce to the condition of a desert vast track of the Southern State of America.
The smallest way it can assist in creating a proper recognition of the condition of the amputates, as found in countries like this, associated with it - my efforts will be amply justified.

(Signed) Mary A. Boole
IV. Note on an Active Principle Derived from a Species of "Lecodendron"

Nat. Ord. Proteaceae.
Protection of the future

Condition: If the counterparty is in default, the counterparty will be subjected to additional sanctions.
Note on an Active Principle Derived from a Species of Lecythisdendron—
Not. ord. Botanicum.

About a year ago while travelling in the district of Worcester (S. Africa) my attention was directed accidentally to a plant growing in great profusion everywhere about. I had in passing along plucked a twig, and commenced to chew some of the leaves. The taste was intensely and persistently bitter. So much so that I could not get rid of it for a considerable time. It struck me that the plant in question might perhaps contain some valuable bitter principle, which, if it could be demonstrated valuable properties of any kind, would have the great advantage of being procurable in any quantity.
L. canc. I. Scutandula protea
from which Proteus was
spewed — J. HAND
I accordingly proceeded as soon as possible some portions of the plant in question for examination. Botanically it was familiar to me. It belonged to the family of the Proteaceæ, and, evidently, a Leucodendron. However, I forwarded portions of the plant to Prof. Mrs. Boas, the Curator of the Botanic Gardens, Cape Town, who kindly examined them for me, with the result that he confirmed my opinion as to its botanical nature. Having no flowers or fruit, he could but determine its exact place in the wide spread family of which it is a member.

Habitat of Plant: It grows in great profusion in the Worcester District. I found species of it almost everywhere since my attention became directed.
towards it— In the Flats near Cape Town it is allied Species abound. The soil is generally sandy, where it grows, it seems to prefer comparatively moist neighbourhood.

Among the ferns it is known from its height (the fully grown shrub attains about 8 or 10 feet) to be
branches of "Lang Beuntes" (Long Legs).

The botanical characters are those essentially of the Proteae
Lemodendron. The Ketta portion of the plant is present in all its parts, but is
prevailing in the leaves.

It is present moreover in most of the Proteaceae that
I have since had a chance
of examining.

The leaves comprise 2/3
Of the whole weight of the plant—

Chemical Examination. As the

flower principally contained

the bitter principle of the

plant, these were alone used

in very attempts in isolating

it. Many experiments were

tentatively made with the

object of finding it— I need

not say that almost a many

failure followed— Indeed the

medical men in Europe, the

difficulties of such an investiga-
tion here can scarcely be realized.

Without more special knowledge

than that possessed by the

average student of medicine,

without direct help of any

kind, without laboratories, with

such appliances and methods only

as one’s ingenuity, or special

orignies sufficed, it seemed
A difficulty which we have in
to commerce, and the condition under
which an attempt at initiating the
principle had to be made, and to
be kept active for practice at
the times it was not easy for
us to find the leisure necessary
for the research. Each stage
of
the experiments was interrupted
by
some particular action.
almost as if I should have to stop my search — Only the determination not to be beaten if it could possibly be prevented gave me the courage to go on, until success crowned my efforts.

I need scarcely say that my small excursion in the direction of Chemical Research, brought with it — a crop of valuable lessons and experiences which will if in the future I undertake anything of the sort again, not be without its reward. I hope so.

Without recording my failure, I shall content myself with stating briefly, how ultimately it was found best to procure the object of my search. The very initiation of the process, when I knew what it was, seemed a matter upon which failure was not only in the process simple, but it will be
seen that it can easily be adapted to the manufacture of the body on a large scale.

- About ½ lb. weight of the dry leaves were taken and infused for 24 hours in boiling water. The infusion obtained was strained off, and the residual leaves again infused for the same time in another batch of water, strained also and added to the first.

The mixed infusion was now evaporated over a water bath, to a syrupy consistence. Rectified Spirit was added to separate to some extent resinous and other impurities of the solution. Filtered.

The filtrate was set aside to crystallize. Crystallized crystals much contaminated with resin and coloring matter, separated from the mother liquid.
These crystals were now washed with cold distilled water & redissolved after working in boiling water. The solution so obtained was digested with animal charcoal, again filtered, & allowed to crystallize. Where impurities still seemed to be present, the crystals were again washed & filtered through charcoal & allowed for the last time to crystallize. The Crystalline Body of which I forward with this a small quantity was the result. Of this it was found roughly that the leaves contained 2% of their total bulk.

I should observe here that the infusing & evaporation were all conducted in porcelain vessels & the vessels were employed, & the plant-like all the Potassium plants, contains a
a large quantity of Tannic Acid. This Tannic Acid can always be rid of by cooking in distilled cold water, it being considerably more soluble than the bitter principle of the plant. The crystals it will be seen are beautifully pure, perfectly white and colorless. Their taste will at once establish their identity with the bitter portion of the plant — microscopically they resolve themselves into beautiful hexagonal prisms, much like the crystals of Gallie Acid.

Chemical Character of the Crystals —

Solubility in Cold Water 1 in 24

Very soluble in boiling water

But reprecipitate immediately on cooling —
Solubility in Ether 1 in 40
Insoluble in Chloroform

Reaction - Neutral

The method of preparation will already have indicated the non-
alkaloidal character of the crystal.
Boiled with a few drops of sulphuric acid neutralised and tested with
Fehling's Solution, they showed
the presence of copper.
The body is therefore a neutral

Glucoside
Boiled with 1 volume of
ammoniacal odor was discernible.
Ether taken up the principle from
an acid solution or what
amounts to the same thing
If to an ethereal solution added
to water in a test tube, 1 drop
Ammonia be added drop by
der, the action principle
is precipitated from the ether

Extract ether into the reagents
water leaving the ether tasteless. An addition of acid again to neutralize the alkali restores the taste to the ether to a considerable extent. A solution of the principle fails to give a negative result with Gold Chloride, Natrium Chloride, Acetate of Lead, Iodine in Solution of Potassium Iodide.

The addition of Perchloride of Iron gives a purpleish blue color. Vitriolic Acid Cold gives an immediate yellow color.

The same reagent added to calcium give a negative result, but after two days digestion, a yellow color also results. From an aqueous solution, the principle is not precipitated by Platinum or Antimonium Carbonate, a proof of its non-identity with Tannic or Gallie Acid.

A solution plus Achromate of Potash +
Sulphuric acid yields a green color with the color. Tannic acid and Gallie acid treated with these reagents yields a red color.

When burnt it produces the vapor from the crystals. When burnt it impacts a tickling sensation to the throat.

So far I had carried my experiments. When it seemed time that it might be profitable if I instituted some differential experiments between it and salicin. These results are here. For convenience sake I shall call the principle in the following table subsequently Proleacin.

<table>
<thead>
<tr>
<th>Sol. of Proleacin</th>
<th>Sol. of Salicin</th>
</tr>
</thead>
<tbody>
<tr>
<td>Given with HNO₃</td>
<td>Given with HNO₃</td>
</tr>
<tr>
<td>Ferri Perchloro.</td>
<td>Ferri Chloride</td>
</tr>
<tr>
<td>Purple blue color</td>
<td>Purple blue color</td>
</tr>
</tbody>
</table>

b. Cold Sulphuric Acid - b Cold Sulphuric Acid - No reaction. A verdish red color.
Act of Protecin gives with sulphuric acid and potassium bichromate, a green color with no particular odor. Act of Salicin gives with sulphuric acid and potassium bichromate, a red color, which changes to green with a smell of methyl orange.

These results then go to show that we have to deal with a body which seems to have chemical analogies to Salicin, but which is not Salicin. They further indicate a still closer relationship between it and the glucoside saligenin, one of the products of the decomposition of Salicin. But there are differences of reaction which seem to indicate some difference of composition. We may therefore provisionally conclude that Protecin is a neutral glucoside, having strong chemical relationships to but not absolutely identical with...
Salicylic acid and salicylic acid hydrate.

For determining the exact chemical nature of the compound an accurate qualitative and quantitative analysis is necessary. These I hope to procure later on when more leisure offers itself. For the present I am forced to content myself with the analysis I have described above.

Physiological actions. To determine this accurately also more time than I have at my disposal would be necessary. Better than in this matter indeed I hope that I may have been allowed some time in the future to recur to kind cooperation of one or other of my European colleagues, who has better and more accurate means at disposal than in a country like this it would be possible to command, who would bring moreover to this study...
of the body (Protecin) in this connection.

Next I kept knowledge of a kind which the average busy general practitioners cannot lay claim to.

With a view respectfully to determine the Physiological action I instituted some experiments which as far as they go at least have the advantage of careful observation and accuracy.

**Experiment I.**

Two Frogs were taken - under the skin of the leg of each were injected 4 grains of Protecin dissolved in water. The same result followed in both. Immediately after the injection the respiration became much hurried, soon (in a few minutes) a diminution followed. It then became intermittent and extremely slow, death ensued 20 minutes after injection - the nervous system appeared to become excited - small stimuli causing considerable jumping about; subsequently it became depressed, complete paralysis seemed
present for about five minutes before death. The eye first protruded much but became subsequently depressed. 

Post mortem. The heart was dilated. 

Death probably occurred from a paralysis of the cardiac or respiratory centre. 

Experiment II

Feeling that the mechanical action of the fluid injected might have had something to do with the production of some of the above symptoms, I next injected into another frog an equal volume of pure water. Result—negative. 

The same frog was afterwards used for part of

Experiment III

Here into one frog were injected two divided doses of 2 grains each at an interval of fifteen minutes. 

The frog manifested the same symptoms as before described.
but twice longer (for two hours) — into another frog were injected two 2 grain doses of Salicylic Acid — no incapacity or symptoms. Burned but no death. Potassa was therefore more active physically than Salicylic Acid.

On the human organism I have not as yet tried Potassa. Its chemical relations suggest the same important uses of the Salicyl Compounds.

So far the present attempts as may consider demonstrate the fact that these salts in the wide spread use of Potassa, an agent which is worth and study, which it all events may prove a valuable addition to our Therapeutic resources.

When opportunity offers I hope to continue my enquiry, for as yet little has been developed.
will not be deemed altogether devoid of interest

[Signature]