1861

James M. Free

1/5 L. Stoughton. The best account of pre-party et. p. 7.
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A Treatise on the Woburan Poison

James McFee
The arrangement that we propose following in the present consideration of that interesting subject, the venomous poison, is the following:

First, a brief consideration as to its preparation; secondly, as to its chemical properties; thirdly, by its physiology as determined by experiments on animals, and lastly, its efficacy in extirpating the venom for which it has been proposed as a remedy.

And first, as to its preparation. It would, perhaps, be more curious and interesting than profitable, on instructive, if we were to review the accounts given of this substance by different authors, from the time of the salt of Ralis in 1063, who seems to have been the first to introduce it to public notice, down to a more recent date.

Some of these earlier writers, it must be owned, were described graphically enough both as to the action of preparing the poison, but all of them seemed to have furnished, or to have seized the least if it within much exaggeration. We shall therefore pass over the notices of this poison by these earlier writers, and at once turn towards...
to the time of Humboldt, whose description of the preparation of the venom, appears to be the first on which any degree of reliance can be placed. The above writer was present during the preparation of the poison at Esmeralda, and states that it is derived from the bark and albumen of the wooden Esmeralda, a species of ice-wood belonging to the strychnine family. He describes its preparation as follows:

'A cold infusion is first prepared by pouring water on the fibrous matter which is the ground bark of the venom. As a yellowish water filters during several hours slowly, drop by drop, through the leafy funnel. This filtered water is the venomous liquor, but it requires strength only when it is concentrated by evaporation, when it masses in a large earthen pot. The Indians from time to time invited us to taste the liquor, its taste, more or less bitter, increased when the concentration by fire has been carried sufficiently far. There is no danger in this operation, the cornea being destruction.'
Wanderings in South America p.51
only when it comes into immediate contact with the blood. The Sebacine, therefore, which are disengaged from the pancreas are not fatal, notwithstanding what has been asserted on this point by the missionaries of the Orinoco."

This extract, with another vegetable juice of a very glutinous character, which is probably added merely to give consistence to the substance, constitutes the Canard. It states that another species of the poison prepared entirely from the root is the active ingredient. Waterton in his "Travels in South America" A work containing many interesting details, and written in a style peculiar to the author, himself, gives a very graphic description of the preparation of the poison, and the manner in which the Indian acts about collecting the necessary materials, which, according to him, are the following, the hoar-pine, a little root, or two bulrush plants, two species of poisonous cactus, some strong Indian pepper, and the fangs of the Cabaui and cona-concha snakes. These different substances
the Macedon Indian prepares as follows. He selects the hoopoe vine and cuts out into thin shavings, and puts them into a kind of caldron made of leaves, then he rolls over an earthen pot and pours water on the shavings; the juice which comes through has the appearance of coffee. When a sufficient quantity has been procured, the shavings are thrown aside. He then bruises the bulboous stalks and syrups a proportionate quantity of their juice through his hands into the pot. Lastly the following are added: pepper, coriander, and cattle grass. It is placed then on a slow fire, and as it boils more of the juice of the hoopoe is added according as it may be found necessary, and the smoke is taken off with a leaf; it remains on the fire till it is reduced to a thick syrup of a dark brown colour. As soon as it has arrived at this state, a few arrows are poisoned with it to try its strength.

And then Mr. Waterton describes the superstitious notions which influenced the minds of the Indians in connection with the preparation of this poison. For instance, the shell when
It has been prepared in bottles or as pellets ever afterwards; the man who prepared it is not allowed to partake of food for a certain amount of time afterwards, and one indeed who had formerly agreed to prepare some of the poison for his detention, declined afterwards to do so, because he feared his body was approaching her period of confinement.

The poison, after being prepared as above is put into a small pot, of the most ingenious construction, carefully covered over and kept in a dry place. It is occasionally warmed over a fire to prevent the effects of dampness.

Schomburgk states that he had an opportunity of witnessing the preparation of the poison both by and Indians. According to him, three species of the strychnine enter into its composition, besides six other plants. An animal (matter of any kind) was used in its manufacture. He asserts that the poison is prepared entirely from vegetable substances, the chief of which is the bark of the strychnus toxifera.

Schomburgk states, that the poison used for weapons by the North American Indians varies.

with every tribe. He declares that notwithstanding all assertions to the contrary, animal poisons do enter into the composition of the arrow-poison, used by the Indians of Peru.

Deltos' repeats Wharton's account of preparing it, and states that the Indians test the virulence of the poison by shooting arrows charged with it into trees; if the leaves drop off or in three days it is deemed sufficiently powerful.

Mr. Bonnier, relying on a work by Dr. Reynolds, states that 'there is a true and a false poison; the two are very different, but extremely difficult to distinguish from each other. The true poison presents, moreover, several species. This substance is therefore not always obtained of the same strength; it comes from different countries, and is extracted from one of several plants, which contain one identical principle; the character of which is to cause death when injected into the blood, and to be innocuous when taken into the stomach.'

"It is well known that it is not always prepared from the same plants, nor from plants..."
of the same nature. It has even been shown by Mr. Chomberk, that one kind of poison is obtained from a great number of plants, almost as great as the number of ingredients entering into the composition of their incand.

Amongst the plants used are some starchy, but the rest has not as yet been determined botanically. It is doubtful whether snake poisons are mixed with it.

We suspect that Chomberk, whom we formerly quoted, is the authority whom the above writer refers to, if so he misrepresents Chomberk's statements. He does not say, 'The same kind of poison is obtained from a great number of plants,' but rather, that a great number of plants enter into the composition of the poison. The subject of the innocuousness of the poison to the stomach will come under consideration at a future period.

Considering the account of the different authors we have quoted, as well as many others whom we might mention, and cannot resist coming to the conclusion, that a great deal of uncertainty prevails, both as to the substances which
into the composition of the poison, and also as to the mode of preparing these substances. It would seem that each separate tribe has a poison of its own, which differs probably in both materials & preparation. The animal matters are used into the poison of certain tribes, there can be little doubt, and as songs of snakes, ants, &c., but how far these influence the activity of the poison is another very different question. Nothing is more natural, then to think, that these Longo Indians, noticing the mortal effect produced by the stings of certain venomous animals, would seek to take (as they thought) advantage of these poisons, and apply them to some useful purpose. It is most likely, however, that supposing they introduced the real poisons of the different kinds of snakes, it would, during the various processes of boiling &c., which it undergoes, be far modified; shall we say decomposed? that it would lose its venomous character. But we shall find that the more savage illiterate Indians, following the dictates
of this own Hindu reasoning, commits a mighty blunder in this matter. We are told by writers that the fangs of snakes enter into the composition of the wound, but we know that the fangs of snakes are no more poisonous than the teeth of any other animal; that the poison of the creature does not reside in its fangs still, but in a special gland it possesses for that purpose, the the Indian, when he introduces the fangs into his poison, introduces what is in fact quite an innocuous substance, and it is not at all unlikely that those writers who mentioned the poison of snakes as being introduced into the poison gland mistakenly, in reality mean the wound fangs. As it is very improbable that the Indian, supposing he wanted to introduce the snake fangs into the wound, would know where to find or look for it. For need he wonder at this as a thing and be found in our own country, not at a thousand and intelligent people also in other respects, who actually believe that the poison of the serpent is located in its fangs.

We think, however, that misunderstanding the
discrepant statements of different writers regard-
ing the composition of this substance, we are
inclined to conclude, on the authority of those
burgh and others, that the extract of the tay-
ker is derived from the base of the tree, while
never other substances may be added along
with it. That there are other ingredients
added, by at least some of the trees, there can
to no doubt, but that these had little or
nothing to do with the activity of the poison
is probable from the reasons already given.

The above named plant is a Phanerogamen
one, belonging to the class Dicotyledons sub-
class Caudifolae and to the order Ergania
but this order has lately been divided
by the same into three distinct Subordi-
ne the Ergania, Epigeleo, and Tracheblad.
In the latter of these the plant under con-
sideration belongs. It corresponds with
the other best known member of the order
in many of its properties, but differs from
them remarkably in its action upon an-
imals, causing paralysis instead of tet-
anic convulsions as we shall subsequently see.
that in preceding we allow the plant to be the principal active ingredient in the wormwood, for as far as I am aware the active life of the plant otherwise than in the form of wormwood, has not been tried, except incident one or two experiments by one of the Schönherr's and the exact symptoms produced have not been detailed. This is all that can be said with any degree of certainty, at present about the composition of wormwood, till future investigations throw more light on the subject.

Leaving now the subject of the composition and preparation of wormwood, we must come to consider what is known with regard to the chemical composition & properties of i.e. Benzinger states that the wormwood in his possession, was ignitable to heat, and dissolve in water, alcohol, hydrochloric acid and typical ammonia, also in blood and saliva except as small portion which subsisted both in the spines and aqueous menstruum and consisting, as he thinks, of earthy particles foreign to the composition.
Examen Chimique des Cahiers Précis des Indiens de l'Amérique. Annales de Chimie et de Physique
It united with acids with formation of a change of colour. On mixing it with alkalis, no alteration was perceptible, but the colour changed from a reddish to a yellowish brown. A few grains mixed with as many grains of human blood, entirely prevented a separation of serum and coagulamentum, and the whole mass continued in a state of fluidity.

The first reliable and thorough examination made of the poison, and one which even yet has not been excelled in completeness, was that of M. F. Rouxin and Boas-en-Gault. The specimen examined by these chemists was obtained from the Rio Segre. It was a solid extract, black, of a viscous appearance, of a brown colour when reduced to powder, and of an intensely bitter taste. This bitterness was unaccompanied by acidity or sharpness. It burned with difficulty, and in consuming gave off no colour of organic nitrogenous substances. It was but slightly soluble in sulphuric ether, more so in alcohol, forming a beautiful
red and very bitter tincture. In water it was
soluble to a considerable extent, forming an
intensely bitter infusion, of slight acid reaction
to litmus paper.
By further investigation Mr.核实 and Auten
gault arrived at the conclusion that no tannin
was present. They however obtained an alkaline
principle soluble in water, for which the name of
caesarin had been proposed. They obtained this sub-
tance by the following process.
The barks was reduced to powder, and treated
repeatedly with boiling alcohol. The alcoholic
extract was evaporated, and the residue treat-
ed with water, which dissolved the active prin-
ciple, leaving nothing but a little resinous mat-
ter. The aqueous solution was then decolorized
by animal charcoal, and treated with infusion
of galls. A beautiful whitish-yellow flaky
precipitate was thrown down.
The precipitate thus obtained was well wash-
ed, heated to solution in water, and dissolved
by the addition of stannic acid. The acid liquor
was then super-saturated by magnesia and
filtered. It was again evaporated to dryness.
Weisen in Reihenorden. Von Richard Schomburgk
Bänd 1-3, 1852
and the residue dissolved in alcohol. This solution was concentrated and spontaneously supereeded to a syrupy consistence. It was then further concentrated by evaporation in vacuo.

This obtained, the Caramina was a solid transparent mass, of an excessively bitter taste, and possessed in an eminent degree all the virulence of the poison. It was not crystallizable, was of a pale yellow colour, and strongly attracted moisture from the atmosphere.

It formed salts with sulphuric, nitric, hydro-chloric, and acetic acids, none of which were crystallizable.

The above named chemists are of opinion that the normal acid of the poison is the Acetic.

The results of the examination made by Mr. Boulin and Houngrange were subsequently confirmed by Mr. Pelletier and Pelletier.

Huntley also examined the poison and chemically.

By adding nitric acid to the aqueous solution of the substance, he obtained an abundant precipitate soluble in boiling water. This was taken from the filtrate, boiled with magnesia, and then evaporated to dryness. The extract thus
Comptes Rendus, t. oo. XXX VIII. 1854, p. 411

Steinert was then treated with alcohol to remove it from any soluble salts of magnesium, and the solution again evaporated to dryness. By this means a yellowish-brown extract was obtained, possessing an alkaline reaction, but endowed in an eminent degree with the toxic principle of the hormone. Steinert does not regard this extract as at all pure. He afterwards employed both the bichloride of mercury and platinum to effect the precipitation, but with no better success, a yellowish-brown extract being still obtained.

Steinert also convinced himself that the extract contained cortinide. He also found sugar, gum, resin, extractive matter, tannic and gallic acids, and traces of saline combinations with organic acids—probably the tannaric and otalic. He was unable to find the least trace of stychein.

Dr. Reinard of Chicago and Dr. Green of New York, in a communication to the French Academy of Sciences, expressed their opinion that the poisonous action of the hormone is probably due to the action of certain peptides, but in a subsequent paper Dr. Green doubt to the
evidence of animal poison in the substance in question.

We regret, very much, that owing to the small quantity of the poison at our disposal, we have not been able to enter into the very interesting subject of the chemistry of the poison. This part of the subject is one of the greatest importance, because if the poison is to turn out useful in the treatment of certain diseases, for the care of which it has been prepared and tried, then it would be of the utmost moment to have the pure active principle, because then we would have a standard preparation to go by, and this is the more necessary, seeing that the different kinds of the poison met with, seem to differ so much in their preparation & strength. In fact without such a standard, we can never be sure, while administering the remedy, of the nature of the substance we are dealing with, unless its effects have been tried first by experiment on the lower animals.

But while much remains yet to be made out regarding the true chemical composition of
of this substance; one thing at least seems determined, from the researches of the different individuals, who have directed their attention to the subject, viz., that no strychnine exists in the boweria, at least such has not as yet been detected in it. We shall subsequently see, that physiological experiments on the lower animals would also seem to indicate the absence of strychnine, notwithstanding the assertions of many authors to the contrary.

We shall now enter into a consideration of the subject proposed in this paper, under the third head, viz. A dissertation of the physiological action of the boweria, as determined by experiments on the lower animals. Experiments had been performed with this substance upon animals, as early as 1743, when de la Condamine's experiments were performed. Subsequently Brockesly, Heissenst, Axtens, and other made experiments with the substance in question, but as many of these only experiments were performed with no definite
Philosophical Transactions vol. XVIII. part A, 1820

Philosophical Transactions part I. 1811, p. 192?
ject in view; and as no great dependance can be placed on the statements of some of those authors, who seem to have at least suspected the action of the poison. For instance, Black

nextly experimented on a cat with the wound by inoculating the animal with it. The cat expired in about half an hour. An hour after

wards, the heart was pulsating, and it continued to beat for two hours after the animal's heart was cut off. He found the poison to kill a small bird in the moment two drops of it, in solution, were placed on the tongue. We

shall pass over the researches of these early writers, and at once come down to those of Sir Benjamin Brodie, who was reckoned the first to enter on the considerations of this subject with any definite aim, and with anything like correct physiological views. The subject of his first experiment was a light pig. A small quantity of the wound in gauze was applied to a wound in the side of the animal. Ten minutes afterwards, he was unable to walk, then he became quite motionless, except from slight occasional convulsions. In gauze
second insensible, the respirations were labour
ed, and at the end of fourteen minutes from
the application of the poison, the respira-
tion had entirely ceased, and he was ap-
parently dead, but on opening the thorax,
the heart was found acting 70 times in a
minute, circulating dark coloured blood,
and it continued to contract for several
minutes afterwards, on dissection no pre-
teratural appearances were observed
in the veins; nor was there any other ap-
pearance in the limb than would have
arisen from an ordinary wound.
In this experiment the exact quantity
of the poison introduced, is not given, but
this fact is that, instead of its being, as he
calls it a small quantity, it was a large
one, as he shall read by and by.
In the next experiment Mr. Brodie made
a wound in the side of a guinea pig, and
introduced into it about 30 gos. of the wound
powder. At the end of twenty-five minutes
symptoms too to place similar to those
which occurred in the former experiment
and in thirteen minutes more the animal was apparently dead, but the heart continued to contract 108 times in a minute, and by means of artificial respiration, the circulation was kept up for more than 20 minutes.

Sir R. Brodie says that "the result of the experiments he had made were similar to those just described. The heart continued to act after apparent death, and the circulation might be kept up by means of artificial respiration. It is evident," he says, "that this poison acts in some way or another on the brain, and that the cessation of the function of this organ is the immediate cause of death." He says that the best mode of applying the poison is dissolved in water to the consistency of a thin paste, and that when the poison was applied in quantity, it sometimes began to act in 6 or 7 minutes, never more than half an hour elapsed from the time of the poison being inserted to that of the animal being affected, except in one instance when a ligature was applied on the limb.
What strikes us very much, in these experiments, is the exceedingly large quantity of the poison used, and the length of time the symptoms required to develop themselves. In the first experiment, the quantity injected is not mentioned, but provided the same poison, as regards its quality, was employed as in the second, the quantity must have been large indeed; in the second experiment we find that with even such a large dose as 30 grs. no symptoms were produced before 85 minutes had elapsed, and seeing that in the first experiment the symptoms of poisoning made their appearance in the course of ten minutes, we must conclude that the dose was considerably larger than that used in the other case by 80 grs. Now unless a poisoned pig is more difficult to poison than other animals of its size, and even much larger than it, we shall see that from our own experiments on the subject one grain must be considered a very large dose. We may easily imagine the quantity which must have been employed in those cases where Dr. Brodie tells us the poison began
to act in six or seven minutes. There is little


doubt but that the poison used in these exper-

iments, was of a very inferior quality.

The author of the above named experiments

tried to account for the comparative feebleness of

the poison, by stating that the poison which

he employed had been preserved for some

years, which he thought would account for

its being less active, than it has been described

by those who had witnessed its effect when

in a recent state. This explanation will

not account for the inertness of the substance

employed. Mr. Hiff states that in some

experiments performed by him upon rabbits,

he found that the poison retains its power

at least twenty-seven years. When moistened

it killed a rabbit in a few minutes.

Dr. Taylor says that unless kept dry it is

apt to become weakened in its properties; this

however is a doubtful statement. It is upon

the whole, most likely that the specimen

of poison used by Sir B. Brodie, was one of

those inferior preparations of which we for-

merly spoke.
Third were other experiments performed by this
same author, with the substance in question,
but we shall defer these, as well as those of
the waterous, till we come to discuss another
point, in our enquiry, viz., a consideration of the
alleged antitoxin for the wound.

We must come to the researches of Virchow
and Hunter. From numerous experiments per-
formed by these observers, they deduced the
following conclusions.
1. That the wound, even after having been kept
dry for five years, is still intensely poisonous.
2. That the physiological action of the wound
is in harmony with the chemical ana-
lysis which denies the presence of strychnine.
3. That wound therefore does not belong to
the class of tetanic poisons, but like opium
induces stupor, and although it causes
slight convulsions acting in cats, there is
never that lethargy, nor tetanus most triumen-
tine.
4. That it induces paralysis of the voluntary
muscles, with at the same time long-con-
tinued action of the involuntary muscles
(heart, intestines)
5. That warm does not appear to produce death by absorption from the external surface of the body, but only when it is absorbed through a solution in the entire fluidity of the animal tissues.

6. That in poisoning by warm, congelation of the fibrine of the blood takes in the same manner as though the animal is killed by mechanical means, and that death takes place not so much from any direct result of the poison, but indirectly by its causing the cessation of the respirating process.

With the first third of these conclusions we entirely concur, but we do not altogether agree with the latter third. The 4th conclusion is that warm induces paroxysms of the voluntary (muscles) with, at the same time, long-continued action of the involuntary (muscles). The idea conveyed to us is that the poison induces long-continued action of the involuntary (muscles), it would be more correct to say that the involuntary muscles are unaffected by it, except as far as it affects them through...
medium of the voluntary muscles, owing to its eye
put on the process of respiration to.
As to its not being able to produce death by
absorption through the skin from the exter-
normal surface of the body, and only when ab-
sorbed through a solution in the continuity
of the animal tissues, is a statement which
we are inclined to doubt, provided the sub-
tant be applied in a proper manner or
certain precaution adopted. These experi-
ments, as to the absorption of the poison from
the external surface, have principally, so
far as we are aware, been tried on frogs; but
Mr. Bernard has pointed out the fact, which
has subsequently been corroborated by others,
that absorption is very retarded if not al-
together prevented in the frog owing to the con-
stant exhalation of a viscous protective mucus,
which is abundant when the frog is in
or just removed from the water, and which
is scarcely observable when the frog has
been long out of that element. The amount
of moisture in the system of the frog has
much to do with this power of absen-
substances presented to the surface of his body. When the frog is dried, more or less, by long exposure to the air, his body eagerly takes up the moisture which is present in any part of the surface. When, on the other hand, his body is thoroughly moistened, and the supply of water is in contact with the larger part of the surface, the frog to absorb an aqueous fluid of a high specific gravity than water from any one part of the surface is considerably lessened.

The same thing holds true in the respect to substances introduced into the digestive canal, the amount and rate of absorption being regulated, more or less, in proportion to the amount of moisture in the system of the frog. It is easy to see how these facts if not attended to might, and often has led to fallacies, when experimenting on this animal, with the view of determining the capability of certain substances, being absorbed from the external part of his body.

The conclusion which the above experimenters have come to under the sixth head.
Comptes Rendus, tom. XXXI, 1860, p. xii
viz, that the poison does not affect the coagulability of the blood, does not correspond with our own observations on the subject. We found that when animals poisoned with the poison, were opened at a considerable interval after death, the blood was in every case found quite fluid; and this is in accordance with the statement of many other experimenters. This non-coagulability of the blood is easily accounted for. The animal to all intents and purposes dies of asphyxia, and it is well known that the blood of animals dying in that manner is slow to coagulate.

The next series of experiments are those of Bernard and Belzey. It is stated by them that animals poisoned with this substance died without tetanic spasm, there being only a few slight contractions of the muscles of the skin, face, and body. They found that in animals poisoned in this manner, there was a total annihilation of all the properties of the nervous system; the reflex movements were altogether lost.
The blood was found constantly black, congealed with difficulty, and did not become red on exposure to atmospheric air. Their conclusion from these facts is that the action of the poison is very similar to that of the vipers and that the analogy is still stronger from the circumstance that, like the latter, it may be introduced into the stomach with impunity. Thus experimenters then endeavored to find out what it was that destroyed the activity of the poison when introduced into the intestinal canal. This they did by mixing different parts of the poison with gastric juice, bile, like pancreatic juice, and then injecting the poison mixed with one after another of the above substances into the circulation of animals, but the poison was still found to be as active as ever. Thus it was proved that it was proved that it was not the action of any of these secretions, which (under the poison) innocuous when introduced into the stomach, but the secretion still remained undetermined, what it was that had
had this effect. They thought that the inertness of the poison depended on the fact that the gastric does not allow the toxic principle of the poison to pass through it. The following experiment is adduced as tending to establish this view.

The fresh gastric mucous membrane of an animal, recently killed, was adapted to an endoscopy in such a manner that the mucous surface was on the outside. Into the instrument was then placed a solution of sugar in water, and the whole was plunged into an aqueous solution of poison. At the end of three hours although endosmosis had been effected, as shown by the elevation of the level of fluid in the tube, it was proved that the liquid contained therein possessed no poisonous quality, showing that the active principle of the poison had not been transmitted. It was, however, determined that by allowing the arrangement to stand for a long time, the mucous surface became so attacked as to permit the entry
Mosis of the poisonous agent.
Sulphur, however, contrary to the results obtained by Dr. Bernard and Ploeg, ascertained that the poison, when introduced into the oesophagus or stomach of certain animals, as fogs, turkeys, and toads, produced death. Cogswell, arrives at the following conclusions: that poison is a poison when when swallowed, that it acts primarily as a stimulant and secondarily, or as it may be termed, speculatively, as a sedative, paralyzing the functions of the nervous system both locally, when it is immediately applied to the body, and constitutionally after it enters the circulation.

We must come to Kolliker's investigations, the most important of his conclusions are the following.

He found that the poison acting through the blood destroyed the excitability of the motor nerves, the terminal branches losing their excitability in a few minutes, whilst their trunks did not become affected for an hour or two later. It is of
opinion that the sensory nerves are little if at all affected.
When introduced into the system through the mucous membrane of the intestinal canal, Kolliker found the morphia to act more slowly than through a wound, and that a larger dose was required. When applied to the skin of frogs he found it altogether inoperative.

With reference to its effects upon the heart it was determined that in amphibia this organ was but little influenced, as it continued to palpitate for many hours after poisoning was established. When to paralysis of the pneumogastric nerves, it was somewhat quickened in its action. He concludes therefore, that the ganglia remain unaffected. The lymphic heat is soon ceased to move.

When applied locally to nerves, morphia in concentrated solution was found to extinguish their excitability, but only after a considerable time. Applied direct to the brain & spinal cord it was
altogether without effect

The main result of Bernard's investigations are the following.
1. That all reflex movements cease a few minutes after poisoning. The heart continuing to beat for a considerable time.
2. That poison is not absorbed from the mucous membrane of the stomach during digestion, bladder, or from the conjunctiva of mammals, but is readily taken up from the pulmonary and nasal mucous membranes of these animals. When introduced into the oesophagus or gizzard of birds, it is rapidly fatal. Applied to the dry skin of frogs, it acts slowly but surely. In contact with the wet skin of these animals, it is not absorbed.
3. Poison abolishes the function of the motor nerves, but does not affect that of the sensory nerves. Innervator irritability is rather augmented than diminished.
4. That poison kills the nerves from the periphery to the center, acting in this way conversely to strychnia.
5. That it causes death by arresting the process of respiration, thus inducing asphyxia.

We shall now give the result of some experiments performed by ourselves with this poison.

Exper. 12. The subject of this experiment was a large full-grown rabbit. The animal being brought into a warm room, 2 gns. of the genuine poison was made into a paste with water and injected into the cellular tissue of the left axilla. The poison was introduced at 12:45 a.m. the animal struggled a little while it was being introduced, but afterwards showed no signs of uneasiness. For about two minutes at the end of that time it began to stagger and in a few moments was quite insensible on the floor. There was no appearance of spasm or convulsion of any kind, except a few very slight twitching movements of the lips, the pupils were enormously dilated; and in a few minutes (before respiration had entirely ceased) the heart was beating very quickly and pretty strongly.
The animal was allowed to remain in this state for about five minutes, when the thorax was opened. The heart was found acting regularly, and perhaps quicker than natural, although the exact number of pulsations in a minute had not counted. It continued beating in this state till 16.10m., or about 2 hours and a quarter from the time the operation ceased. When it became more irregular, slower and weaker, especially the ventricle (right), and in another five minutes the ventricular action stopped entirely. Up to this time the right side of the heart only was exposed, the animal lying on its left side; on looking to the left side of the heart both chambers were still. The right auricle, however, continued contracting still regularly, although slower and weaker than before, and it was noticed that the auricular appendage that was the last part of the auricle that contracted each time. This went on till 16.35m., or twenty minutes after the action of the ventricle had ceased, when the
Circulatory action became irregular with the exception of the appendix, which still continued to contract regularly, although gradually getting weaker and slower in its action; five minutes (now and the circulatory action was at an end, except the appendix which also grew irregular and still weaker and slower than before). Now the five minutes, or about two hours from the time the poison was introduced, all has still. The heart was found full of venous blood, but all the other vessels were found to possess quite a normal appearance. The lungs being collapsed & quite free of blood. The heart & spinal cord were carefully examined but no mark. appearance could be detected in either of them, except that the venous pinnas were engorged. The spinal cord, (notable elongated & appeared quite healthy). The wound where the poison was introduced was also examined but nothing peculiar was noticed, with (respect to) as much of the poison, was there still unabsorbed, as might be sufficient (probably)
to poison half-adulted rabbits; the cellular head round the wound was packed with it although it was only here injected. We had a little mistake in this experiment, as to the amount of the wound which should be made, from relying on the statements of previous experimenters. In fact we had no idea of the strength of the substance we had to deal with; the result was that poisoning was so speedily produced, that there was but little opportunity afforded for observing the symptoms produced by the poison, as they manifested themselves. In fact the animal lay insensible at our feet, before (as we thought) the poisonous effects had time to manifest themselves.

The phenomenon which most engaged our attention, however, and in which we were not a little interested was the order in which the different parts of the heart ceased to contract. It is well known among physiologists that the right annulus of the heart is the part which last ceases to obey the stimulus of the blood when the animal is dying, but we do not recollect if ever seeing any allusion being
made as to the right auricular appendix, being the ultimo atrium. Whether this be the
vein or natural root of cessation of the
heart's contractility we leave our readers to
judge, at all events in this experiment the
appendix continued contracting distinctly for
the space of five minutes, after all the the
parts of the heart had ceased to contract. This
phenomenon was not watched in any of our
subsequent experiments.
Exper. II. At 12h. 53m. one grain of the poison, mix-
med into a paste with water, was injected
into the axilla of a large cat, for the space
of three minutes afterwards, the animal went
about the room, apparently suffering no un-
consciousness, kicking its legs in the howl until
the poison was introduced alternately, but
it was observed that at the end of about
two minutes the pupils of the eyes began
ning to dilate; the heart regular but rather
quick; in another minute the animal began
to show signs of unconsciousness, raising its foreleg
alternately, which seemed to yield under it as if
somewhat paralysed, and making apparently
involuntary efforts to get itself into a comfortable position; in two minutes more the animal fell on its left side, being the side on which the poison was introduced; the pupils of the eyes were enormously dilated, and for the space of one minute there were occasional tremors or slight convulsive movements of the limbs, with deep gasps at intervals, as if the animal attempted to draw an additional quantity of air into the lungs; at 3 minutes before one the convulsive movements of the limbs had entirely ceased; the heart's action quick but regular. At about 3 minutes after the poison had been introduced all respiratory efforts had ceased, at no time during the experiment was anything like titanic convulsions observed. The throat was opened five minutes after the creature had ceased to breathe, but the heart's action had entirely ceased, nor could it be excited to contract by vibratiting it in various ways. We ought also to state that after the suspension of the heart, none of the ordinary methods employed for this purpose, could excite any reflex action in any part of the body; galvanism was not
employed. On examining the brain, spinal cord, and the different thoracic & abdominal organs, nothing unnatural was noticed, the appearances were much the same as in the previous experiment.

In this experiment I thought that from the animal falling down on the side into which the poison was injected, the action of the side might possibly have something to do with it; that the muscles of that side were first paralysed, but subsequent experiments did not confirm this view; the occurrence was quite accidental. With regard to the action of the poison, why the action of the heart should cease so suddenly in this case compared with the previous one, as well as all the other experiments which had taken place, was quite (unable to explain). Certainly it was not the quantity of the poison introduced, because it will be observed it was only half as much as was used in the first experiment.

Exper. III. The subject of this exper. was a cat as in the former instance. Half a grain of
The wound moistened with hot water injected into the pith of the animal about 10.40. At the operation the animal began to kick itself in a sort of playful manner, and for five minutes showed no symptoms of uneasiness but at the end of that time began to show signs of uneasiness & discomfort by frequent changing his position, and moving one after the other of the forelegs in the spread of another minute the animal fell, with occasional undulating movements of the limbs; right pressure as yet in another minute the convulsive movements were more marked & frequent, and violent efforts at breathing were made. Pulse very quick in first minute not counted, at the end of two minutes the pulse had fallen to 140 per minute, with still slight convulsions of limbs and twitching of the muscles of mouth, viscera still perfect to other third minute elapses and then the pulse was 84 and very irregular, there were violent efforts at respiration, but the convulsions of extremities all but ceased, pupils not as much dilated as at first, twitching of the muscles of the mouth still continued.
and the animal makes violent and ineffective attempts at respiration; in another 5 minutes all movements had entirely ceased. No attempts were made to breathe, the pulse was as weak and irregular that it could not be counted. And at 11 o'clock, twenty minutes after the introduction of the poison, the animal was dead.

Exper. IV. One-fifth of a grain of the poison (in the substance not mixed with water) was introduced into the arilla of a large cat. The pulse was counted before the experiment proved to be 100 per minute. The poison was introduced at 11.28, and only five minutes elapsed before the animal was affected much in the same way as in the former experiments, only when the extremities of the animal were pinched convulsions after movements were observed. Five minutes after the commencement of the exper. the pulse had fallen to 25, all the judgeinent phenomenon were much the same as in the former case, and the animal died at 12 minutes before 1 o'clock, less than a quarter of an hour from the introduction of the poison. The appearances seen after death corresponded...
with those mentioned in former experiments, it will be observed that in all these experiments (either opisthotonus, empassion, or any thing simulating tetric convulsions occurred), which would justify the statements of many writers on this subject, viz. that the active properties of poison are due to the presence of strophanthid, or in other words that strophanthid is the active principle in the poison, and that mere slight convulsions movements of the limbs seen in most cases but those in no way resemble the tetric convulsions and spams so characteristic of the action of strophanthid.

The activity and virulence of the poison can easily be judged from the last experiment, when only one-fifth of a grain was employed and that too in the solid form, the poison was produced by the substance being manifested in five minutes. We had no hesitation in saying that one-twentieth, perhaps much less, of the poison would have been quite sufficient to kill the animal. When this
Phil. Transactions 1811, p. 196.
in contrast with Sir Benjamin Brodie's experiments already referred to, when the poisonous symptoms were observed till twenty-five minutes had elapsed, notwithstanding twenty grains of the poison was injected, we think we are justified in our conclusion that the poison must have been of a very inferior quality. The last four experiments show how easily whole the poison is, from the short time in which the poisonous action was manifested, although the substance was introduced in the solid form.

Though what medium does this poison act, or how is it absorbed when applied to a wound? This question has been elucidated in an admirable manner by Dr. B. Brodie that the poison acts through the medium of the brain or nervous system, there can be no doubt whatever, but how does it get there when applied to a wound in some distant part of the body. The above writer says that 'a poison applied to a wounded surface may be supposed to act on the
brain in one of three ways.
1. By means of the nerves the poison takes internally.
2. By passing into the circulation through the absorbent vessels.
3. By passing directly into the circulation through the divided veins.

Mr. Brodie instituted a set of experiments to ascertain in which of these modes the poison acted. To determine whether it acted through the medium of the nerves he divided all the filaments of nerves in the axilla of an animal, dividing even the cellular tissue to make sure of every filament of the nerves being cut; in this way leaving the artery and vein entirely isolated; he then made two wounds in the corresponding leg of the animal, and introduced some of the poison into the wounds. In the course of 34 minutes the animal was dead. On dissection after death not a filament of nerve was found divided. In this way he demonstrated the fact that the poison did not act through the nerves.
To ascertain whether the poison was taken up by the absorbents or lymphatic vessels, the thoracic duct was tied where it joins the subclavian vein, in a dog; a quantity of the poison was then introduced into a wound in the corresponding side, and it was found that the animal died in the ordinary time. It has thus shown that the poison must be absorbed through some other channel than the lymphatic vessels. The same experiments, again, having isolated the artery and chief vein of a limb tied with a ligature round all the other structures of the limb leaving the vein or artery unincluded, the poison introduced into a wound below the rest of the ligature had the usual effect; but when a ligature was tied round the whole limb, it was found that the action of the poison could be modified or altogether prevented, according to the degree of tightness of the ligature, and the length of time it was allowed to remain on the limb.

From the above experiments to Dr. Brodie concluded that the poison, like other poisons of the same class is absorbed directly into the circulation through the twisted ends of the vein.
Fred Times & Gazette. 1859. Vol. 18. p. 661
Perhaps the above result is not altogether conclusive, as the experiments did not prove but that some of the fevian might be taken up by the lymphatic vessels, although the chief part undoubtedly is taken up by the veins.

I doubt that even hardly be any doubt but that the lymphatic vessels do take up the fevian.

Although it would seem almost unnecessary, after what has been already stated in a former part of this paper, to say anything more about the fevian of staphylin in the moroeus, still it is strongly urged by stating that the activity of the substance depends on the presence of the above echebath. It might not be out of place to say a few words about the subject.

In one of the Medical Journals, we find it stated that in the Martin-Preyer and Buehner, there the existence of any antagonism between moroeus and staphylin. If they say, these persons to are identical, they nevertheless suffer only in slight modifications of their effects, which modifications almost always disappear when the doses and means of their administration are ceased. This would the
strychnine) causes convulsions, by rendering the spinal marrow more excitable; and in order to obtain this result, it is indispensable to poison the spinal marrow before the extremities, while, in order to have no convulsions with the strychnine, the extremities must be poisoned before the spinal marrow. From which it follows that the poison and the strychnine pass into the extremities of the motor nerves. The writer of the above article goes on to state that these results of Dr. Martin Meyer and Baccarans are most important and if correct, they directly contradict the doctrine on which his views are based, the treatment of tetanus by poisons; and they show that this poison ought to be altogether banished from therapeutics, as adding only another danger to the danger of the tetanus itself. In addition, the added to this difficulty is the fact of the poison (may also twist) all the observations of Bernard, that maternal animals are only with great difficulty brought under its influence, the condition
of absorption being apparently altered in them.

As to the latter remark, he had made in objection to
the idea of the nose and in teneur, we hold them
to be an objection all for the contrary they
pand to as an argument in its favour because
the difficulty with which the mucilaginous and
tonic animal is brought under the influence
of the poison shows that there is no tolerance
of the remedy induced by the disease. It is in
fact what we see in the case of most of our
most powerful remedies; for instance, in acute
stenic inflammations we have tolerance of
bloodletting to such an extent as would render
fetal in health. Are we to employ this as
an argument against the use of bloodletting
in such cases? The same remark apply to
antimony, opium, alcohol, and all our most
powerful remedies.

We do not think that the observation
of Dr. Martin-Pragnard and Bouicard and the
shadow of proof in their favour as to the
identity between the nose and the
mouth, has been proved beyond doubt that
I went to Col. J. 1856 p. 648.
there is an antagonism between the two poisons as seen from the following fact:
Dr. Harley poisoned a frog with strychnine and while the tetric symptoms were very severe, he introduced three grains of cerei into the cellular tissue. The limbs became perfectly flaccid in ten minutes, and no irritation induced spasm.
Dr. Harley conceiving the animal to be dead opened it two hours afterwards, but to his astonishment found the heart acting rhythmically. The next day the heart was found still beating, and the circulation in the web of the foot slow but regular. Fifty hours after the experiment the animal showed no signs of life all the time & yet the circulation continued.
In a footnote to the above article Dr. Harley states “touacali and strychnine had in fact the effect of reciprocally neutralizing each other, according as the one or the other poison is in excess. Being occupied with experiments on the subject I shall only add that...”
1st. A frog was poisoned with 1/40 of a grain of strychnine. Three minutes after tetanus was strongly marked, he was punctured with 1/20 of a grain of bournali. In 7 minutes tetanus disappeared.

2nd. A frog was poisoned with 1/40 of a grain of strychnine bournali. Three minutes after he had become perfectly insensible to 1/40 of a grain of strychnine was injected, in five minutes he became tetanic.

3rd. For 1/20 of a grain of bournali, and 1/40 of a grain of strychnine were injected into the abdomen of a frog. At five minutes past one; at ten minutes past it became very tetanic; at half past one (twenty minutes afterwards) it became perfectly flaccid, and the next day it appeared perfectly well. This is the more curious, Dr. Stanley remarking as the dose of strychnine in this case was certainly more than was sufficient to kill it. Thus it would appear that strychnine might be used as an antitoxin for bournali, and bournali for strychnine.
With the view of determining this question we lately performed the following experiment. About 2 grains of a solution of strychnia was introduced by means of a catheter into the stomach of a cat; the animal subsequent to emplying the contents of his stomach by vomiting, but not till the action of the strychnia began to manifest itself. In a short time the animal was tetanic to the highest degree; the spinal irritability was so much heightened, that in the intervals between the violent tetanic jerks which succeeded one another rapidly, the slightest touch would immediately throw the animal into convulsions, and make his body bound away from the irritant agent. One fifth of a grain of the poisonous poison was injected under the skin, and about five minutes afterwards the spasms entirely ceased, the animal dying in about ten minutes afterwards, perfectly free from convulsions, and with all the usual symptoms of poisoning with strychnia. We might add also many other facts ill
Janet Vol. II. 1859, p. 345
tending to show the antagonisms between the bromine and styrchnine; but we think the above with what has been previously said on this point and quite conclusive. This leads us to say a few words regarding the efficacy of bromine as an antitoxe to styrchn. This is a subject of the utmost importance, and one that demands much attention than has been heretofore bestowed upon it. When we consider what the poison (styrchnine) is, a substance for which no antidote is known, and a substance also from which fatal results are but too commonly observed, then we repeat that the importance of discovering an antidote for the above poison cannot be exaggerated.

Dr. Wray, who has devoted as small degree of attention to the subject, remarks, "you may, perhaps, remember that in 1856 I pointed out, in the pages of your journal, the antagonistic action of bromine and styrchnine. Attempting these experiments to show that these two substances have the power of reciprocally (neutralizing each other, according
as the one or the other poison is in excess. The conclusion I then drew from my experiments was, that wormsia might be used as an antidote for strychnine. Since 1856 I have frequently repeated these experiments, and on several occasions have succeeded, by means of wormsia, in saving the lives of animals to which I had administered strychnine in poisonous doses.

It has been objected to the use of wormsia as an antidote for strychnine, that we are only substituting in the place of one poison another, and a more deadly one in its effects; it is undoubtedly true that this is the case, so far as our knowledge of the wormsia at present goes, but it is not. We think that this is the reason for not inquiring into this subject more thoroughly. Our first duty is to find out the active principle in the wormsia, whatever this may be, and in this, get a standard substance to experiment with; then by repeated and careful experiments on the lower animals find out how much of the wormsia principle, a certain quantity of the strychn...
mine would require to counteract its effects. In this manner, we would arrive at an accurate knowledge, with respect to the quantity of ipecacuan that ought to be taken in a case of poisoning by strychnine, where the quantity taken of the latter was known.

The whole question at present does not rest upon whether the ipecacuan will act as an antidote to strychnine, that is already made out, but upon the amount of ipecacuan that a given quantity of strychnine will require to counteract the effects of the latter, without at the same time producing a poisonous effect by the antidote employed. While there exists as at present such a difference of activity, between the various kinds of ipecacuan (as with it, it would be next to impossible to arrive at any definite result of this kind, unless we find out the principle on which the activity of the poison depends. At the same time if such investigations were entered into, we have little doubt, but that good results would follow. It might even become a question whether we would not be justified

in going a step further, if, as we have every reason to believe, we have an infallible remedy in artificial respiration, in cases of poisoning by poison; for instance, given a case of poisoning by strychnia, we know that it is otherwise as hopeless a case, that we have no antidote which we can administer, unless we try the poison, and we justifiably in pithing the poison until the symptoms of the strychnia poisoning give way, and if it should so happen, that poison act as the antidote is supposed to be developed, before the end in view be attained, trust to artificial respiration for counteracting the latter effect.

That there is nothing irrational in this being the one poison to counteract the effects of the other is shown by the fact, first point cut out by Sir W. Bell, that opium, a poisonous substance, acts as an antidote to belladone, another poisonous substance, &c. &c. Cases of this kind have been tried with success by Smeas tenor, &c. &c. Where to counteract the actions of belladone, doses of opium were
American Rice Gazette Vol. VII Nos. 5 and 7, No. 6, 1855-56
administered (which would otherwise prove poisonous, with the effect of counteracting
the effects of the belladona).

Arsenic is said in the same way to be an
antidote to the snake poison.

We shall next inquire (very briefly) into the
antidotes for this poison itself. Many antidotes
are spoken of by the various writers on this sub-
ject, such as garlic, sugar and salt, sugar
canes, vinegar, brandy—all of which it may
be stated at one end of the scale whatever.

The mineral acids when mixed with the
poison before injecting it seem to render it
innocuous. In Brainerd and treat! Consider
the mixture of tartar of potassium and bicarb
as an antidote, when mingled with the
poison in solution. But we believe that art
ificial respiration is the only means upon
which, at present, any degree of reliance can
be placed, in order to save the animal, after
the effects of the poison have manifested
themselves. Unless as formerly stated strychn
and may be found to act in the form of an
antidote.
Philosophical Transactions, 1812

So far as we are aware Sir P. Bridgewater was the first (who actually restored an animal to life) by means of artificial respiration. The experiment has since been confirmed by Galton and many others. It would be very interesting & instructive to notice a few of the most remarkable of these experiments, but really our time and space are now both so limited that we can do nothing more than refer the reader to the works where these experiments are to be found. We may mention that we performed two experiments with the view of determining the efficacy of artificial respiration, one of these in a dog and the other in a cat, and although the artificial respiration could not be continued a sufficient length of time, owing to the little time at our disposal, and other circumstances interfering, still we saw enough to convince us of the value of the above means for restoring the animal; in the case of the two experiments the circulation was kept up actively for the space of three hours after the animal had ceased to
breath, and was as strong as ever (the circu-
lation was) when the experiment had to be
discontinued. It seems now, however, a well
established fact, that artificial respira-
tions, if skillfully & perseveringly employ-
ed, is a sure way of bringing an animal
round from the effects of the poison.
Another important subject remaining for con-
sideration viz. whether the poison is, as
is held by many innocuous when taken
into the stomach? WB shall not waste
time here by entering into the various discus-
sions regarding this subject, but at once
give the result of our own experiments on
the subject. WB may remark in passing
that the experiment of Bernard and Ploenz
formerly noticed, in which they endeavored
to explain the innocuousness (as they thought)
of the poison by taking it into the stomach,
was a very unsatisfactory one, and to say
the least of it a very unfair trial of the
power of absorption of the mucous coat of the
stomach. It will be remembered that this
experiment was made by means of an in-

Densimeter full of a solution of sugar in water, a piece of fresh gastric mucous membrane was adapted to the instrument, and then the whole was plunged into an aqueous solution of pnumic; the conclusion was, that because none of the pnumic passed through the mucous membrane, the thing happened in the living stomach; but surely we are not to believe that the absorbing power of the gastric mucous membrane is nothing (more or less than) the pnumic action seen in a dead membrane, such as a piece of bladder, which when adapted to the end of an densimeter, allows of the passage of fluid in or out through it, according to the degree of consistence of the fluid on one side or the other. There is surely something (more than this in the action of the living gastric mucous membrane); surely some vital force regulates its action, as well as that of all other parts of the body.

As we think that our own experiments are quite conclusive on this subject, we shall now state them as shortly as
A cat was kept fasting for at least 18 hours, so that the stomach was quite empty. Ten grains of morphia dissolved in water was then introduced, by means of a catheter into the stomach; the animal for some time afterwards did not show the least sign of uneasiness, but greedily devoured two or three small pieces of mutton, which were given him at intervals. At the end of 18 minutes, however, the symptoms of poisoning began to show themselves, and the animal died in the usual time, manifesting all the symptoms which were observed, when the poison was introduced by injection into the cellular tissue. A question now arose as to whether the mucous membrane of the stomach or oesophagus might not have been injured by the catheter, while introducing the poison, and whether the poisonous effect might not be explained in this way; with the view of determining this point the animal was opened after death, and the mucous membrane of the stomach and whole tract of the oesophagus examin
ed with the greatest care, but no abnormal appearance could be observed; in fact, the mucous membrane was quite healthy in every way; the stomach was quite empty except that it contained the small pieces of food given to the animal shortly before its death, and these had already partly undergone the process of digestion. In a second experiment the animal (not) received about a grain of the poison concealed in a small bit of flesh; the stomach was not previously empty; it had no effect whatever; two hours afterwards two grains of the poison were given in the same way as before, but this time the animal showed a little inclination to take the bit of flesh, feeling the bitter taste of the poison; at length however, he did take it; it had no effect this time either. The animal was now allowed to remain over night without any food, and next morning when it was brought to the mesh was quite empty; two grains of the poison were given in a bit of flesh as formerly; this time the animal showed more
dislike to taking the poisonous meal then on the former occasion; but owing to the hunger at least it did take it though reluctantly. After a greater and hour or twenty minutes had elapsed, the animal was evidently affected; it lay in a listless half asleep state, and showed none of the liveliness which it had formerly manifested; this, however, passed off after a time and the animal was quite well, none of the more evident symptoms of poisoning were observed, as in former cases. It was our intention to increase gradually the dose of the poison, till we would find out how small a quantity was sufficient to cause death, but unfortunately our stock of wounder was at an end, and I knew our experiments had to end with it.

This then was satisfactorily proved, that wounder does kill when administered into the stomach, as well as when injected into the cellular tissue, but a much larger dose is required in the former case. It was also shown that the state of the stomach, whether full or empty, influences the action of the poison, the action of the wounder being much less powerful in the former than in the latter case.
Mayo's Outline of Physiology
We now come to consider the subject proposed under the third last head, and what must be considered as the most important part of the investigation, viz. considerations as to the efficiency of horseradish in the treatment of certain diseases for which it has been proposed as a remedy; and first as to its efficacy in the treatment of cure of tetanus.

Mr. Lewell was, as far as we are aware, the first to try the effect of this substance in the cure of tetanus. Giving the disease in horses as the result of irritation of the nervous system, Mr. Lewell conjectured that if a horse in tetanus was destroyed by poison, which acts by suppressing nervous power, and life was then to be restored by artificial respiration, the nervous system on resuscitation taking place, might possibly be free of the original morbid irritation. Reasoning thus, Mr. Lewell tried the following singular practice. A horse suffering under a severe attack of the tetanus, and before jaw, the mouth being too firmly closed to admit the introduction of either food or medicine, was inoculated on the fleshly part of
Lampert October, 1953, p.348
the shoulder with an arrow point coated with the wound's poison. In ten minutes apparent death was produced. Artificial respiration was immediately commenced & kept up about four hours when resuscitation took place; the animal was apparently soon fully recovered, and eagerly partook of hay & corn. He, probably, was too abundantly supplied with food during the night. The consequence was over distention of the stomach, of which the animal died the following day, without, however, having the slightest recurrence of tetrac sympotoms.

Dr. Hely* states, that through the kindness of Professor Berrill of the Royal Veterinary College, he had the opportunity of trying the effects of woundi on a horse labouring under severe attack of tetanus. Although, he says, he did not proceed in saving the life of the animal, I nevertheless saw enough to convince me of the value of the remedy. Indeed, I was so convinced of its beneficial effects that I would have tried it on a boy labouring under traumatic tetanus. The how shortly afterwards saw a boy
'First Lines of Gazette vol 12 1859 p 343. Also Zemut vol 2 1859 p 300
With Dr. Partridge, tried the disease (not yielded to other remedies). This subject of the effect of quinine in the treatment of certain invertebrate diseases, is attracting the attention of medical men more and more every year, and from the lower animals the experiment has been extended to the human subject, both on the continent and in our own Country; but the matter lies by no means received that degree of attention which its importance demands.

In order to come to some correct idea of the advantage of the remedy in the cases in which it has been tried, in the human subject, and at the same time to ascertain how far the substance had received a fair trial in those cases in which it succeeded, as well as in those in which it failed, we deem it our best plan to review shortly all the cases in which it has been tried, and thus red what we can (much of them).

The first trial in the human subject was made by Mr. Bell of Bahrain. The result of this latter trial was given in a paper read by Mr. C. Bernard.
before the Academy of Sciences, August 29, 1859.

It seems that M. Véris, relying on some experiments performed by Bernard in 1850, instituted a
lengthened series of experiments in 1852, which led him to conclude that there is a direct antagonistic action, as regards their effects on the nervous system, between strychnine and morphia; in other words, that the effect produced on animals by one of these poisons can be neutralized by the other. And this was the foundation of the following experiment.

M. Véris, who during the late war was attached to the French military Hospital at Trier, having observed several cases of tetanus among the wounded, in which the use of opium, ether, and the usual remedies had failed, deemed it a fitting opportunity for the application to the human subject of the experience he had derived from his observations on animals. His first experiments were made on two soldiers, both suffering from tetanus; in one the affection was of four, and in the other of five days standing. Both were in a state of semasphysia, and their recovery atto-
gether hopeless; notwithstanding this, however, the application of moro tea was followed by a certain amount of muscular relaxation, and of general relief to the patient, although both in the end terminated fatally.

In a third case, the experiments were more successful, and the patient was discharged from the hospital perfectly cured. The subject was a French Sergeant who had been wounded on June 14 at the battle of Magenta, by a ball which had caused an incomplete fracture of the first metatarsal bone of the right foot, with lacerations of the tendons and soft parts. He entered the hospital on June 16 under the most favourable circumstances. Three days afterwards the ball was extracted, and the operation relieved him of much of the pain which the presence of the foreign body had caused him. On the third day after the extraction of the ball, stiffness of the neck (made its appearance) attended with difficulty of moving the jaw and the head. There were also slight convulsions, but these were of short duration.
The day following the jaws became firmly locked, rendering it at times impossible for him to open the mouth. On the 18th the occurrence of trias, opisthotonos, and other characteristic symptoms, lift (no doubt as to the serious character of the disease, and it was declared by all the surgeons attached to the Hospital to be general tetanus. The condition of the man was so serious and alarming that Mr. Bella deemed it necessary to bleed him from the arm (for the purpose of relieving the asphyxia which he was threatened. He then, after having freely incised the wound, administered a powerful dose of opium, but these measures produced no good result. On the afternoon of the same day, he decided on the application of a vioce to the wounded foot; the dose being in the proportion of two grains of vioce to an ounce and a third. First applied in the form of compresses; the strength of the solution was gradually increased until it reached the proportion of twenty grains of the vioce to nearly three ounces of water, three quarters of an hour after the application
of the weaker solution, and half an hour after the (more) powerful was employed, a visible diminution of the tetanic rigidity was the result, followed by such complete muscular relaxation, that the patient was almost immediately able to drink, to take soup, to urinate, and to sit up in bed. At the commencement of the treatment it was remarked that as soon as the peculiar physiological effects of the moroara began to pass off the tetanic spasms reappeared with as much violence as ever, and that the wounded leg was always the first part of the body affected by them. After three days of this treatment a large blister was applied to the thigh, in order to secure a more extensive absorbent surface, and the solution was applied to it as well as to the wound of the foot. For four days the dressings of the entire abating surface were renewed every three hours, then every five hours up to the twelfth day, when they were reduced to twice in the twenty-four hours. It was remarked by Dr. Bells that the wound of the foot and the raw surface produced
by the blister were not irritated by the application of the arrow, as they had already rapidly.

The arrow, which for the first eight days succeeded in rendering greater the interval between the paroxysms, and in diminishing their intensity, ended by making them cease entirely; and on the 10th of July the patient was able to leave his bed for the first time without experiencing any convulsive shock. On the 15th he went out for an hour and on the 26th of the same month he left the hospital perfectly cured.

We may here remark, that in the discussion which followed the reading of this interesting paper, Ch. Bernard, Cloquet, Robert de Lamailly, and others, regarded the happy termination of the case due to the curative influence of the arrow.

Case IV. The patient, a robust man, was attacked by tetanus after a contused wound in the sartorial region, and fracture of the bone. He was admitted in the Chauti hospital, (under the care of Dr. Brancic), whose opinion...
surgery, when tetanus occurred, thought of wood
arsen, which has been successfully employed
by Dr. Hebra of Vienna. Dr. Bulfinch was requested
to superintend the case of the wound, which
was injected into the cellular tissue in sol
(tun), each injection being of the value of
about a tenth of a grain of wood arsen. Later in-
jections contained as much as half a grain
of the poison, and, finally, as much as
a whole grain. Its particular effect was
produced; the septicemic attacks increas-
ed in number and severity, and the patient
died eight hours after the use of the arsen.

Case I. It relates to a young man, twenty-four
years of age, who met with a gun-shot accid-
ent. The second toe was carried off, and
the base of the third (much crushed). The
wound, a fortnight afterwards, was in good
condition, when the patient was carried a
distance of a few miles. On the next day
the wound became painful, severe agony in
the region of the right temple, was complai-
ned of, and became set in two days after-
wards. An ointment (with chloriform)
was fixed on the jaw, and (much and glue were given internally). The attending surgeon, Dr. Andre and Iser, worked to save (woorden), but could not procure any. The case went on from bad to worse, and empysematous tracts being well marked, Iw. Cheyne-Rose was called in consultation. The patient was ordered two grains of (woorden) in four ounces of water, a tablespoonful to be taken every hour; the wound to be irrigated every second hour with a solution of four grains of (woorden) in seven ounces of water, and to be immediately covered with lint. Some improvement took place after the first (woorden) ful, when the jaw was considerably less rigid. The prescription was rigidly carried out, and at ten o'clock next morning the patient was (much) better; the trismus, however, persisted, though only recurring in fits. The solution of (woorden) for the wound was now carried to the strength of six or eight grains for seven ounces of water; and in three or four days, the dose of the remedial agent in the mixture was increased to three and four grains to the four ounces of water. The patient slept on
steadily improving, but there was still some

tremors left when the case was brought before
the Surgical Society, twenty days after the
first ticanic symptoms, and sixteen after the
commencement of the treatment by the wound
are poison.

It was objected to the efficacy of the wound
in this case, that the wound taken by the
mouth has had no show in the improvement
as it is well known that the poison does not
act when placed in contact with the mucous
membrane of the stomach. Whether it has
any show in the improvement or not, the
grounds of the above objection are unfounded
as we have already satisfactory proves, that
the poison does not act when placed in contact
with the mucous membrane of the stomach.

Case 71. Another case of ticanic has occurred
here in the services of Mr. Follic, at the Hospital
Fisher, in which wound was tried, but without
out success, as the sequel will show. The
patient, a lad, 16 yrs of age, had sustained
a fracture of the lower third of the radius of
the right arm, accompanied by considerable
arising of the soft parts. He was admitted on October 18, and up to the night of the 3rd of the present month (November) everything went on satisfactorily. On the morning of the 4th, the masticator muscles were observed to be a little contracted together with convulsive movements of the muscles of the jaws. These symptoms rapidly increased in gravity, and at eight in the morning there was some difficulty in opening the jaws. Inspiration was also difficult; the respiration was abdominal, giving twenty-eight inspirations to the minute; pulse 116.

At half-past eight a.m., ten drops of a solution of horehound (of the strength of one part of the drug to 100 of water) were injected by means of Peraz's syringes into the cellular tissues of the forearm; the quality of the horehound having been previously tested by Mr. Bulfinch, who is deemed an authority on this subject. Each drop introduced by the half-hour of the syringes was extemely slight at three cent grammes. A fresh injection was made every half-hour, the dose being gradually increas
ed, so that at half past two p.m. they had reached twenty-four drops. About half past four a more concentrated solution was employed, but the number of drops was diminished. These injections, like the others, were continued every half-hour, the dose being gradually increased, so that at half past eight p.m. mind there appeared no change in the tetanic symptoms, with this exception, that the muscles were perhaps not quite so hard. A eleven p.m., the jaws could be more easily separated, and a certain amelioration was perceptible. Injections into the cellular tissue of the chest were now had recourse to. The improvement did not continue; the disease, on the contrary, making such rapid progress that, after one more than usually violent paroxysm, he died at half past one p.m., the following day. In connection with this case, Dr. Folliot afterwards, from Dr. C. Bernard certain facts which may in part account for the more success of the course, Dr. Bernard declaring that the action of this poison is infinitely less marked than act
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ministered to the mutilated and suffering animal than when given to the healthy and strong."

The next three cases are those of Spencer Wells and the only tried, we believe, which has been made in the human subject in this country. The first was a case of chronic but severe tricus, going on to osteothorax, appearing a fortnight after ovariotomy in a patient forty-one years of age. The exciting cause appeared to have been a draught of cold air. The great point of interest in the treatment was, that (though the cases had been chronically and epichronically, six grains of the extract having been used in six days. The patient recovered.

In a second case, acute tetanus appeared seven days after ovariotomy in a lady thirty-eight years of age, and proved fatal in three days. The treatment was commenced by acetyl tetrate injection, and wound five afterwards, the softened extract having been inoculated in both arms.

In the third case tetanus appeared four days after a simplified parametrectomy operation
for the relief of protruded uteri in a patient
fifteen years of age. It progressed slowly;
was treated first by opium and ether, after-
wards by hordard, and latterly by chloroform;
the influence of which was kept at inter-
vals for forty-eight hours. The patient
died on the evening this letter was read.

We here now given, as far they are known
to us, the results of all the cases in which
poorerox has been tried for the cure of tetanus,
in the human subject, both in this and in
other countries, some of these ended favorably
others unfavorably. Our time will not al-
low us to go over the merits of these different
experiments in their order; it may be stated
in a general way that no definite conclusion
can be arrived at from any one of these cases
nor from the whole taken together. But while
these experiments have not proved the benefi-
cial effects of the (poorerox) in the treatment of
tetanus, they lead by no means shown that
it has no effect in that disease; on the
contrary, enough has been shown to warrant
a further trial of the remedy, and to entertain
the hope that it may yet turn out to be a very useful remedy in this, as well as in other febrile diseases of the same class. It will be observed that in most of the cases quoted, exception or two, the substance could not be said to have got a fair trial; it was not resorted to till all the other known remedies, generally employed, had failed, and when consequently the system was so much depressed, and the disease so far advanced, that little good could be expected from any remedy, although under a different combination of circumstances the most efficacious. Again we must remember that utensils, applies under a great variety of circumstances; at one time arising from the most trifling injury, at another as the result of the greatest lesion or operation. It is further very probable that there are two varieties of the disease quite distinct and separate from one another as to their cause; the one probably arising from nervous irritation directly, the other arising from a certain materia (mutter which being absorbed into
the circulations acts though it indirectly on the nervous system, however this may be it is quite plain that it would be absurd to begin to treat a case of tetanus after a severe injury, lath venom and anything else as undoubtedly is often done, without at the same time endeavouring to support the system in every possible as it would be to begin to direct some special remedy against the lung in case of phthisis, while the great evil which lies at the bottom of the whole viz. the inflammation of the lung is left unaided for or unattended to. In this respect tetanus must only be looked upon as the symptom of some other (more general) disease, which is as yet to be understood, and therefore the morrow is merely directed against this symptom, while at the same time no means must be neglected in order to keep up the system which is so depressed by the evil of which the tetanus may be looked upon as the result. In other words we know nothing of the pathology of tetanus, we merely observe a certain
combination of phenomena, which we regard as a disease and call by the name of tetanus, but this merely to conceal our ignorance of the subject because tetanus is nothing more than a symptom and should be so treated. It is for these reasons that we object to most of the trials which have been made with the arrows used in the treatment of tetanus, that the arrows was looked upon as if it should single-handed, restore the individual to health, other important means being neglected. While in fact the arrows was put forth directed against a symptom of the disease. Besides in many of these experiments as sufficient quantity of the poison was not employed, there being evidently a tolerance of the remedy induced by the disease. We do not in the case of an acute inflammation regulate the amount of bleeding by the number of ounces drawn, but by the effects produced on the system, and on the disease. The same thing holds true in regard to opium, digitalis and all our most powerful agents. And as it must be with regard to tetanus.
and in the treatment of ténas if it is to
prove of any service in that effect. It is quite possible that the remedy fail
ed in the experiments already so often al-
luded to, just simply because an insufficient
quantity if it was given on the same prin-
ciple as an ordinary dose of opium will have
no effect in many acute diseases, such as
febrifugies, when the dose must be enor-
mosly increased in order that the desired
effects may be produced.
We do not mean to say that any man,
considering the imperfect state of our know-
ledge regarding materia medica, would be justified in using the
remedy to such an extent as to over-
come the ténas symptom irrespective of the
quantity employed; what we mean is that
(as man) is entitled to conclude, from the
failure of the substance, in none of the ex-
periments already adduced, that it has
no beneficial effect in the treatment of ténas.
And that a series of correct experiments
are very much needed on the subject.
It may yet come to be a question, considering the deadly nature of tetanus, and how very little it is amenable to any kind of treat-
ment, whether we might not be justified in infusing it till the tetanic痉挛 give way, and if need be employ artificial res-
piration to resuscitate the individual, that is of course in an otherwise hopeless case.

The process has been proposed in the treat-
mint of hydrophobia, and has been tried in
the treatment of epilepsy (see Edinburgh
Medical Journal for March 1861, p. 82). 123

Must, however, conclude this paper, without
making any remarks on its efficacy in
the above diseases, as we are afraid that
this Thesia has already extended to an
unnatural length.

James Ferriby