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POISONING BY STRYCHNIA,  
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ON

# POISONING BY STRYCHNIA,

WITH

COMMENTS ON THE MEDICAL EVIDENCE

GIVEN AT THE

TRIAL OF WILLIAM PALMER

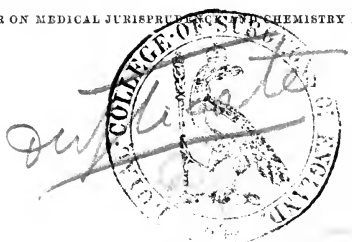
FOR

THE MURDER OF JOHN PARSONS COOK.

Waine?  
BY

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## PREFATORY REMARKS.

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It has been said by one writer in reference to the case which forms the principal subject of this paper, that "the contradictory deductions and 'tetanic complications' of medical professors make wise men tremble, good men sad, and bad men bold: they encourage the reprobate in his onward career of guilt, and doubly arm the secret poisoner with a scientific cause for the commission of crime, whose victims drop like withered leaves in autumn, and fall beneath the influence of his dark and stealthy dealings like a fabled fate." Another observes: "One of the worst effects produced by the trials of the recent strychnine murderers was the impression left on the public mind, enforced by the Press, enforced by the prosecution, and almost sanctioned by the Bench, that *with a little search medical men might be got to prove anything*. And after making full allowance for the learning and integrity of a profession, than which we verily believe no profession contains a larger proportion of learned and honorable men, it cannot be denied that there was some ground for the imputation. \* \* \*

It seems to us that the magistrate whose duty

it may unhappily become to weigh the opposing evidence of medical witnesses, requires to be quite as discriminating of the moral elements which enter either scale, as if he were actually engaged in weighing out the maximum of a virulent poison that might be administered with impunity."

Severe as these censures are, no one can view the facts as proved in the case of WILLIAM PALMER without feeling that to a certain extent they are justly merited. There are few, if any, who can now entertain a reasonable doubt that John Parsons Cook died from strychnia administered to him by William Palmer, or that the criminal has been justly punished for this detestable crime. One witness for the defence swore that the death of Cook could not be referred to strychnia or any known cause. Having assumed, as these pages will prove, an artificial and erroneous standard for the symptoms of strychnia-poisoning, he deposed that the mode of death was unlike strychnia. It came out in evidence that in a clear and well-marked case of death from tetanus, the same witness was unable to distinguish it from a case of poisoning by strychnia, and actually made an analysis for detecting this poison, on the assumption that the man had died from its effects! Another stated he was so deceived by one fatal case of angina pectoris, that he could not distinguish Cook's case from it. A third was unable to distinguish this case from an ordinary fatal case of epilepsy. Two of the chemical witnesses, while by their evidence they were endeavouring to persuade the jury that Cook did not die from strychnia because it was not chemically detected, believed at the time that it was present in the body, but that a sufficiently delicate process had not been used by the Crown witnesses for its ex-

traction! One had expressed this opinion before the trial, and the other has published an opinion, substantially to the same effect, since.

Let the public and the profession reflect upon this kind of evidence, given on the solemnity of an oath which requires a man to state "the truth, *the whole truth, and nothing but the truth,*" and concede to the witnesses who made these statements that credit for sincerity which is their due—and what is the impression that the case of PALMER must leave upon the mind? It can be no other than "to make wise men tremble, good men sad, and bad men bold." Persons selected and presented as witnesses of authority and experience, assert that the symptoms produced by this formidable poison are so little characteristic of its effects, that it is impossible to distinguish them from those of tetanus, epilepsy, angina pectoris, and other nervous diseases. If this be true, then it follows that there is no certainty—no rule for guidance in practice or diagnosis! A person actually poisoned by strychnia may die, owing to his case being mistaken for one or other of these diseases; while another dying from tetanus may be treated for strychnia, and his body after death may be diligently examined for this poison. If the symptoms are so completely undistinguishable as the witnesses for the defence wished the jury to believe, what security is there for life, or what certainty of detecting hereafter a case of poisoning by strychnia? The alleged certain discovery of this poison in a man's liver after death will not cure such mistakes as these, nor will it help a jury to a just verdict on a criminal charge, until this species of hyper-chemical evidence has undergone a closer scrutiny than the exciting circumstances arising out of the Palmer case have permitted.

These pages have been written with a view of showing that the "contradictory deductions" made by the witnesses in this case are more apparent than real. The *facts* are now before the profession, divested of that mystification in which it appears to have been the object of the counsel for the defence to place them; and each member will be able to judge, according to his experience, whether they admit of any other inferences than those which were drawn by the medical witnesses for the prosecution.

GUY'S HOSPITAL;  
October, 15, 1856.



ON

POISONING BY STRYCHNIA,

WITH

COMMENTS ON THE MEDICAL EVIDENCE GIVEN AT  
THE TRIAL OF WILLIAM PALMER.

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BY ALFRED S. TAYLOR, M.D. F.R.S.

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THE attention of the profession and public has been of late especially directed to the subject of poisoning by STRYCHNIA, from the facts and opinions elicited on the trial of WILLIAM PALMER, for the murder of JOHN PARSONS COOK. This trial, which occupied the Central Criminal Court for the unusually long period of twelve days (14th to 27th May, 1856), will be for ever remarkable in the annals of jurisprudence and legal medicine. An Act of Parliament was expressly passed for the purpose of moving the proceedings from the county of Stafford, in which the crime was alleged to have been committed, to the metropolis, in order that the accused might have the benefit of an impartial jury. Three learned Judges, the Lord Chief-Justice Campbell, Mr. Baron Alderson, and Mr. Justice Cresswell—men eminent for their knowledge of criminal law—presided on this occasion. The prosecution was conducted with consummate ability as well as moderation, by the Attorney-General, assisted by four distinguished members of the bar; and among the numerous witnesses for the prosecution and defence, were men whose names are well known to science, and many of whom occupy a high professional position in London, Dublin and Edinburgh. In spite of the most strenuous efforts made to “save the life of the prisoner,” by

the display of an unusual amount of sophistry, by a misrepresentation of medical facts and opinions, by personal attacks on witnesses whose evidence was of vital importance in the case—the last refuge of a failing defence—and finally, by an attempted intimidation of the jury, the prisoner was convicted, and has since suffered the punishment justly due to his crime.

If we seek for an explanation of the extraordinary excitement produced in the public mind by the facts elicited at the inquest on the body of JOHN PARSONS COOK, and at the trial of WILLIAM PALMER, we shall probably find it, so far as this crime is concerned, in the deadly and insidious nature of the poison selected,—the name of which was until then hardly known to the public,—in the ingenious mode of administration, *i. e.*, by the substitution, on two successive nights, of poisoned pills for pills prescribed by a medical man; and in the fact, that the accused was himself a member of the medical profession, a man of education and knowledge, as well as of a certain degree of respectability. The sudden deaths which had taken place in his house during a period of four years—of three children, of a friend while on a visit, of his wife's mother, and lastly, of his wife, for whose murder by poison he was to be immediately tried in the event of an acquittal on the charge which he was then called upon to answer—gave an interest to these proceedings far beyond that attached to an ordinary case of poisoning. With a prevalent conviction of the prisoner's guilt, there was at the same time, an almost universal feeling that his trial for the murder of Cook would end in an acquittal. His legal advisers felt confident of a verdict in his favour. Whence arose this feeling? The crime had been committed in secrecy with consummate skill and art; and the poison to which death was ascribed by the witnesses for the Crown, was not found in the body of Cook. The case, therefore, had to be made out to the satisfaction of the jury upon other than chemical grounds; and this was thought to be a difficulty which the skill and experience of the legal advisers of the Crown would never surmount, although aided by the advice and opinions of many medical men whose names and reputations were well known to the public, who could have no object in coming forward to give evidence on the occasion, than that of upholding the principles of truth and justice.

It was plausibly stated, and widely circulated by a portion of the press, before and pending this trial, that no man can die of poison except poison be found in his body, and that unless the material instrument of death be *always* and *under all circumstances* forthcoming, upon such charges, no man's life would be safe! The varied aspects of disease were described as such that no one could, under any circumstances, trust to symptoms and appearances as evidence of death from poison. The other side of the picture was studiously concealed from the public view; namely, that to men of craft and skill in the medical profession, deadly poisons are accessible, which may destroy life in such doses and under such modes of administration, that, while no chemical tests can reveal their presence in the body, their unlawful use may be surely and satisfactorily indicated by the suddenness, intensity, and peculiarity, as well as by the fatal rapidity of the symptoms which they produce. There are other poisons accessible to professional men, which, according to the mode of administration, may be detected under one set of circumstances, while the tests will wholly fail to detect them in other cases. It was studiously concealed that, while the chemist's art is utterly valueless to aid the law in cases of this description, physiology and pathology will enable the medical observer to detect the crime. If the poison be administered,—by whatever channel it may enter the body, there will be the symptoms to announce its effects. Due caution is of course required in drawing inferences from symptoms, but an equal, if not a greater amount of caution, is also demanded in drawing inferences from the results of delicate and refined chemical processes applied to the solids and fluids of the dead body. Is the chemist more certain of the accuracy of the tests employed in such cases, than the experienced physician of the symptoms? Take an instance in which the symptoms are so doubtful that they might be assigned to strychnia or disease. The chemist demonstrates, as he says, by certain colours, the presence in the dead body of the fifty thousandth or the twenty thousandth of a grain of poison; one of a sanguine temperament will tell you that, beyond all doubt, it is strychnia; a second will affirm that the appearance is equivocal; and a third will tell you that he disbelieves altogether that it indicates the presence of

the poison. This, as it will be seen hereafter, was very much the state of things in the case of Cook; and it is not at all improbable, from the kind of evidence given at the trial, and the bold reliance placed upon infinitesimal results, that had the chemists for the defence changed places with the chemists for the prosecution, the prisoner would have been chemically convicted by his own witnesses. Is it upon a slender reed like this that the public are to be taught to lean to protect themselves from death by poison? Let it be remembered, that if the physician, as a pathologist or physiologist, may be deceived by symptoms, the chemist may be equally deceived by his tests. He may, and often has, pronounced poison to be present when it was not; and he has overlooked it when it was present. What is produced as poison from a dead body, may not be poison at all. The varied results of chemical tests and processes may mislead, and often have misled the most experienced men; and there can be no doubt, that an absolute and blind trust in chemistry, as all-sufficient to settle a disputed case of death from poison in the affirmative or negative, would lead to the most serious consequences. The skilful poisoner, well acquainted with the selection of poisons, their doses and properties, and possessed of a knowledge of compounding them, would escape the penalty due to his crime, while the ignorant assassin who resorted to this species of murder would alone become amenable to the law. Abandon physiology and pathology as sources of evidence of poisoning, when chemistry fails, and no life will be safe. The path of secret murder will be clearly pointed out, to be trodden with impunity by those whose vicious propensities incite them to resort to these means of death. There is a refinement in crime, as there is progress in the arts and sciences; but it is only occasionally that we have to contemplate the perversion of medical knowledge and professional skill to the act of secret poisoning. Miss Abercromby, whose life was largely insured, died suddenly in 1830, having had symptoms of tetanus in a severe form before death. Chemistry failed to show any poison in her body, and the popular idea was then, as now—"no poison found, no death from poison." Her death was referred to cold and hysterical convulsions. No one who has considered the facts of this remarkable case, can doubt that this young

woman was poisoned by strychnia by her relative Wainwright. It was the first murder by strychnia perpetrated in this country ; and, as I believe, completely overlooked by reason of this mistaken confidence in the infallibility of chemistry to reveal the presence of the agent under every conceivable case of death from poison. Twenty-five years have elapsed, and we have had another case under very similar circumstances. So long back as 1823, the French had practically settled the question which in this country led to the concealment of the murder of Miss Abercromby in 1830, and, but for a resolute and determined opinion, based on facts irrespective of chemistry, would have suppressed the murder of John Parsons Cook, in 1855, and ensured the impunity of the criminal. I allude to the case of Dr. Castaign, who was convicted and executed in 1823, for the murder of his friend Ballet by the administration of morphia. I shall, in another part of this paper, give a brief history of this case ; but I may now remark, that the poisoning took place at a country inn—the intended victim was first dosed with tartar emetic—a medical man was called in—he was then dosed with morphia, from which he died. The scientific contest which took place in reference to Palmer's trial, occurred at the trial of Castaign. Six of the most eminent chemists in France made an analysis of the body, and found *no trace of poison* ; and in this instance there had been no tampering with the stomach of the deceased. The defence was that the man had died of some obscure and unrecognisable disease, and that there could be no poisoning, when no poison was discovered in the body by men of great skill and experience in analysis. Only one physician, a man of eminence, could be found to sustain this doctrine ; it was rejected by the Court and jury, and the prisoner was justly convicted.

This false dogma was again brought forward at the trial of Palmer ; and we may expect that in every case of skilful poisoning, in which, either by reason of the poison selected, by the mode of administration, or by subsequent tampering with the dead body and viscera, obstacles are thrown in the way of chemical research,—it will find its professional supporters. It is satisfactory to know, however, for the future security of life in this country, that, on the trial of Palmer, it received no support

whatever from any great names, whether in chemistry, physiology, or pathology, connected with the Medical Schools and Universities of London, Dublin, Edinburgh, Glasgow, Aberdeen, or St. Andrew's.

Although the result of this trial has shown that the popular view regarding the nature of the evidence required for conviction in a case of murder by poison is not correct, and that physiology and pathology may in some cases of themselves supply the deficiencies of chemical science, the question is still one deserving of grave consideration. The popular idea of infallibly detecting poison in the dead, has no doubt arisen from the occurrence of a large number of cases of poisoning by arsenic and other mineral substances, in the detection of which no difficulty has been commonly experienced; because, if the poison were lost from the stomach, it might be easily found in other and remote parts of the body from which no art or skill could remove it, except by the complete destruction of the animal substance. We have yet, however, to acquire experience respecting the detection of strychnia. Chemical tests and processes have not undergone that ordeal with respect to strychnia which they have with respect to arsenic. This is the first case in which a person has been tried for murder in this country by the use of strychnia; and in this instance the perfect security and infallibility of the tests was not so much a question, as whether the poison could be detected under circumstances in which, up to the period of the murder of Cook, it never had been detected. When we find a chemist deposing on oath to the presence of this poison in a dead body in the proportion of a fifty-thousandth of a grain, and the conviction of a prisoner rests on the admission of this statement as evidence, we shall be better able to judge of the amount of confidence that will be placed by the jury, the profession, and the public, in chemical tests applied to such substances as strychnia, than we now are. This transcendental chemical evidence would in such a case lead from life to death, and would therefore require a little more sifting than it received at the late trial, where, as it was employed for the defence of the prisoner, it could only lead from death to life—that is, by its reception, to lead to the acquittal of the accused. It cannot be denied, however, that the conflict of scientific testimony

elicited on this and other points at the trial of this great criminal, has thrown disgrace upon the medical profession, and has created in the public mind a feeling of insecurity in relying upon scientific opinions under any circumstances. It will, I fear, take many years to remove this feeling; but, in the meantime, one great result has been gained. There is now no reason to fear that life will be insecure, because poison may not always be found in a dead body. A skilful assassin will not escape by the application of professional knowledge to the purposes of crime. At the same time, medical practitioners need not dread that a false charge will be raised against them because a patient happens to die suddenly under their treatment. William Palmer was not convicted upon loose coincidences of this kind, but, irrespective of all medical theories, he led to his own conviction by a series of acts which, in the mind of every unbiassed person, were perfectly inconsistent with his innocence.

On the 27th of November, 1855, Mr. Stephens, the stepfather of the deceased John Parsons Cook, was introduced to me by Mr. Warrington, of Apothecaries' Hall. He was accompanied by Mr. Boycott, clerk to Mr. Gardner, solicitor, of Rugeley; and from Mr. Boycott I received a jar containing the viscera of deceased. It is proper to state that this jar had not at any time been in the custody of Mr. Stephens. I was informed by Mr. Stephens that the deceased, who was of the age of 28, and of average health, had died suddenly on the night of the 20th of November, 1855; that he had been slightly indisposed for about five or six days previously, the indisposition consisting chiefly in attacks of nausea and occasional sickness, but without purging; that on Monday night, the 19th of November, he had had a fit, from which he recovered; and that, on Tuesday, the 20th, about midnight, he was seized with convulsions, and went off suddenly. It was supposed that he had taken a pill, or some medicine which had been prescribed for him, on the preceding night; and I was further informed that an aged medical practitioner had been in attendance upon him shortly before his death. From circumstances which were not then stated to me, a suspicion had arisen in the mind of Mr. Stephens that his stepson had not died from natural causes; and, in order to set these suspicions at rest, he requested me

to undertake the analysis of the viscera, and to make a report of the results to him.

It subsequently transpired that, for about a fortnight before his death and pending his illness, the deceased had been in company with a medical man,—a friend and associate, who was with him when he was first seized with sickness at Shrewsbury, when he was attacked with his fatal illness at Rugeley, and, lastly, who was present at the post-mortem examination of the body. The name of this person (William Palmer) was not mentioned, and it was not made known to me until after the analysis had been completed.

The deceased died on the 20th, and the examination of his body was made on the 26th of November. The jar containing the viscera was, as it has been already stated, delivered to me on the following morning. As the case appeared to involve the character, and possibly the life, of a medical practitioner, I requested that my colleague, Dr. Rees, should be associated with me in the analysis, so that, for the satisfaction of all parties, there might be a witness to the proceedings. This was assented to by Mr. Stephens.

Here it is necessary to pause, to show how the initiation of proceedings of this grave kind is sometimes a matter of accident, and how nearly one of the foulest murders of ancient or modern times altogether escaped detection. The examination of the body of Mr. Cook, and the analysis of the viscera were made a matter of private arrangement on the part of the friends of the deceased. Mr. Stephens, the stepfather, was compelled either to allow the subject to pass over quietly, or personally to incur the responsibility and expense connected with the inspection and chemical analysis. The coroner was represented to be a friend of the suspected party, and from circumstances which subsequently transpired, it appeared that he received a present from him pending the inquiry, and a letter suggesting that the verdict should be one of death from natural causes! English coroners are not generally open to a charge of this kind: they frequently show great energy, activity, and independence in the performance of their duties; but either through lack of vigilance or want of zeal, the case of Mr. Cook would have been altogether overlooked but for the course pursued by Mr. Stephens, the stepfather of the deceased. This



gentleman was, however, so placed, that he was obliged either to forego a proper inquiry, or to pay out of his own means the costs connected with it. For the sake of public justice he adopted the latter course. He acted too with singular discretion in the matter. He took care that the jar containing the viscera of the deceased should be at no time in his custody, so that, in the event of poison being found, it might not be imputed to him that he had placed it in the jar owing to a vindictive feeling against the suspected person. The fact that he had, at his own cost, procured an examination of the body and an analysis of the viscera, led to the improper suggestion in the defence of the prisoner, that his conduct indicated personal feeling. The fact was, however, and no one knew it better than the counsel who made this uncharitable suggestion, that had not Mr. Stephens taken the responsibility of the post-mortem examination on himself—a triple murder, of a friend, of a wife, and probably of a brother, would have remained for ever concealed. The conduct of those whose duty it is to procure, for the sake of public justice, a proper examination and analysis in every sudden and suspicious case of death, thus exposed a gentleman of unimpeached character to an undeserved and indecent attack, and created such difficulty in the subsequent analysis as practically to defeat it.

With the jar containing the viscera, a short note of the appearances found after death was placed in my hands. This note was in the form of a private communication from Dr. Harland, of Stafford, to Mr. Stephens. Dr. Harland was present at the post-mortem examination by the wish of Mr. Stephens, and furnished him with this private note at his particular request.<sup>1</sup> This note of the appearances threw no

<sup>1</sup> At the trial, this private memorandum was magnified by Sergeant Shee into a document most importantly affecting the interests of the prisoner, and of the knowledge of the contents of which they, the counsel for the defence, had been deprived up to that moment. The memorandum was delivered to me by Mr. Stephens, with a request that I would not part with it unless it should be required for future reference or corroboration. It was in no sense a public document, but a private note. It was not signed by the other gentlemen present at the post-mortem examination, and merely had the initials of Dr. Harland. Although the learned sergeant might as reasonably have expected to receive copies of every private letter which had passed between Mr. Stephens and myself, he had not the candour to remove the

light on the cause of death. There was no trace of disease, or any morbid change in a vital organ, to explain the illness or nature of the convulsive attack under which the deceased was supposed to have died. The only appearances which particularly attracted attention, were the emptiness of the heart, and the general fluidity of the blood; but there was nothing to show the operation of any poison.

Such was the history of this case, as it was laid before Dr. Rees and myself, before we commenced the analysis, *i. e.*, within forty-eight hours of the time at which the viscera were delivered to me—the earliest period at which we could meet for this purpose. I subjoin from our notes, as an important preliminary statement, the following memorandum :

“*Thursday, November 29th, 1855.*”

“In the jar was found the stomach, *cut open and turned inside out*, presenting over the greater part of its surface a deep reddish colour, especially towards the pyloric end. On examination by a lens, there was no ulceration nor whitish spots seen, nor was there any perforation. There was a feculent odour observed.”

The stomach was lying on the intestines; there were no contents; ligatures had been placed on the two ends of the stomach, probably with a view, in the first instance, of securing the contents, but for some reason, then inexplicable to us, this very proper design had been abandoned.<sup>1</sup> The jar contained only the stomach and intestines. We immediately

suspicion of unfair dealing which he had thus by his unfounded suggestion raised in the minds of the jury.

The reader will observe, that among the anomalies of this remarkable case, there was no regular report drawn up of the appearances presented at the post-mortem examination, and signed by all the persons present at it. This may account for some contradictions on minor points among the witnesses for the prosecution.

<sup>1</sup> Let the reader contrast this with the evidence given by Mr. Devonshire, who removed the stomach, and placed it in the jar. Having stated that he punctured the stomach, and some of the contents fell out on a chair—the question is put—

*Q.* Did you *tie* the stomach up *where it was punctured*, before you put it in the jar, with a *piece of string*.

*A.* Yes.

This evidence, it will be perceived, is perfectly irreconcilable with the state of

made application for the other viscera; and on the 1st of December we received, in a second jar, the liver, kidneys, and spleen, as well as a small bottle containing three drachms of blood taken from the vena cava.

The result of our examination is subjoined :

CHEMICAL EXAMINATION, MADE NINE DAYS AFTER DEATH AND  
FOUR DAYS AFTER INSPECTION.

*Stomach and Intestines.*—The stomach had been cut open, and it was found in the jar lying on the intestines turned inside out. The contents (if any) had entirely drained away from it. There was a brownish-coloured liquid adhering to some portions of the mucous surface or lining membrane. There was no appearance of ulceration, perforation, or other disease. The mucous membrane was generally reddened, and this redness was very strongly marked towards the pyloric or lesser end of the stomach. There was no appearance of effused blood under the mucous membrane; and on examining the whole surface of the stomach by a magnifying glass, nothing of a suspicious nature could be perceived. There was no mineral powder, nor any vegetable matter, deposited on or adhering to the surface. There was no odour of opium, prussic acid, or ardent spirits. The only odour perceptible was that of feculent matter, arising from the stomach having been placed in contact with the intestines in the jar.

The large and small intestines presented, both on the outside and on the inside, slight patches of redness in different parts. The small intestines contained a yellowish-coloured fluid, chiefly consisting of bile and mucus. The large intestines contained feculent matter, without any admixture of blood or any unusual appearance.

A large portion of the intestines was cut open, the contents washed out, and the intestines well drained in distilled water. The liquid thus obtained was found to be a mixture of bile, mucus, and feculent matter. After twenty-four hours, it deposited no mineral or vegetable sediment. This liquid was reserved for special examination. There was no blood contained in it or mixed with it.

*Liver, Kidneys, Spleen.*—The liver was healthy in structure. The kidneys and spleen were also free from disease. The left kidney was smaller than the right, and there was some slight phosphatic deposit in the tubuli uriniferi.

*Analysis.*—The washings of the stomach were first tested by the usual chemical tests and processes for arsenic, mercury, and other metallic poisons. They were found to contain a small quantity of antimony.

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the stomach when we removed it from the jar. There was no aperture tied up. There was no indication of an aperture having been made, for the stomach was completely cut open from one end to the other, and there was no string except the ligatures at the two ends. There is no reason to suppose that Mr. Devonshire has not stated what he actually saw and did; and therefore some person must have interfered with the stomach before it reached us. Otherwise, it could not possibly have been found as we found it.

The coats of the stomach, with the contents adhering to them, were examined for prussic acid, oxalic acid, opium, morphia, strychnia, veratria, nicotina, and conia, as well as for arsenic, mercury, antimony, and other mineral poisons. The result of this investigation was that they yielded only a slight trace of antimony.

The greater portion of the intestines, large and small, was submitted to analysis, and a small quantity of antimony was also found in them. The liquids of the intestines were examined for alcohol by (distillation), and those poisons which might occasion sudden death, but there was no trace of any of these. Antimony was found in the fluids and coats of the intestines in small quantity. About one eighth part of the liver, the left kidney, and spleen, also yielded antimony. The quantity was less in the spleen than in the other organs.

The drainings of the jar in which the stomach and intestines had been placed were examined, and found to contain a larger proportion of antimony than the other parts. The drainings amounted to about two ounces, and consisted of blood and feculent matter mixed.

The contents of a bottle (31ij) enclosed in the second jar, with the liver and kidneys, were found to consist chiefly of blood (putrid). They were examined for metallic poison, and yielded a small quantity of antimony.

#### CONCLUSIONS.

1. That *antimony* derived from some antimonial preparation taken during life, was present in the body of the deceased.

2. That antimony had been absorbed, carried into the blood, and deposited in the various structures in which it was found.

3. That the quantity discovered was small: it might be the residue of a large dose, or of various small doses, taken some hours, or even days before death.

4. That the quantity actually taken by the deceased, cannot be inferred from the small quantity found in the body of deceased, or from the appearances presented by the stomach and intestines.

5. That antimony under the form of tartar emetic may occasion nausea, violent vomiting, purging, and other symptoms of irritation of the stomach and bowels. In large doses it has caused death, and death has been preceded by convulsions.

6. That the viscera of the deceased, so far as they were examined by us, presented no appearances to account for death from natural causes.

7. That antimony as tartar emetic may be given as a safe and innocent medicine, or be used as a poison. We have no evidence before us to enable us to form a judgment as to the circumstances under which antimony was taken by deceased, and whether it was or was not the cause of death.

(Signed)

*Tuesday, December 4th, 1855.*

It will be perceived that the analysis left the cause of death quite undetermined. There was an absence of any accurate knowledge of the duration of the fatal illness under which the deceased sank, and of the nature, course, and progress of the symptoms preceding death. For anything that then ap-

peared to the contrary, the antimony found in the body might have been innocently given, and the deceased have died from natural causes, before the metal had been entirely eliminated.<sup>1</sup>

We were informed by Mr. Gardner, before we sent the report, that the medical practitioner suspected of having caused the death of the deceased, had recently purchased three active poisons, namely, strychnia, prussic acid, and Battley's sedative solution of opium. This information, however, was not allowed to affect or in any way alter or modify the conclusions at which we had arrived. It was enough for us that we had already performed our duty in searching, with the best means in our power, for the poisons indicated, with the result that there was no evidence satisfactory to our minds that any one of them was present. We did not, therefore, feel ourselves justified in assigning any cause of death from the facts then before us. A private note to this effect, addressed to Mr. Gardner, was, as it was subsequently proved, opened by the postmaster of Rugeley, and the contents were communicated to William Palmer. It is proper to state, as much misapprehension has existed on this point, that the report of our analysis was not sent to Mr. Gardner at Rugeley, but to Mr. Stephens at his residence at Kensington. Had it been sent to Rugeley, it would no doubt have shared the fate of our letter: the seal would have been violated, and the contents communicated to the person since proved to have committed the murder.

It will be perceived from this statement, that as no poison was or could be specified when the viscera were delivered to us, our research was necessarily directed to the detection of a variety of substances which might occasion sudden death. We had only the *coats* of the stomach to deal with, and, in spite of

<sup>1</sup> We thought that we had guarded ourselves from any imputation of rashness, by the cautious mode in which these conclusions were drawn up. We accused no one, but simply stated facts, and suggested possibilities which might be confirmed or refuted by evidence subsequently obtained. Let not a medical man, however, suppose that, by any amount of caution, he can protect himself from an imputation of this kind. When he has to deal with men who have no scruples as to the mode in which they conduct a defence, provided they can thereby gain a verdict, he must be prepared not only to hear his motives questioned, but his opinions denounced as rash and unjustifiable.

what some of the chemists retained for the defence assumed, we had not the *contents* before us. The drainings of the jar in nature and quantity did not correspond to the fluid which was said to have existed in the stomach, part of which was said to have escaped through an aperture made accidentally (!) in this organ during the inspection. Thus there were insuperable difficulties to the discovery of any alkaloidal poison, even supposing that all the material in the shape of a decoction of the coats had been consumed in the search for one poison, and that one *strychnia*.

Further, as there was clearly antimony in the body, and the quantity present might become a material question, we considered it to be our duty, after having made search for mineral poisons generally, to use the residue of the soft structures for the separation of antimony and for the detection of mercury. The search for the latter metal was made in consequence of the statement that calomel pills had been prescribed for and taken by deceased shortly before his death. The result was, that antimony was detected in all parts more or less, but there was no mercury. The quantity separated from the parts examined, was estimated at about half a grain.

After the report had been sent, a paper containing a statement by Mr. Jones, the medical gentleman who witnessed the death of the deceased, was placed in the hands of Dr. Rees, by Mr. Stephens, and from a consideration of this, assuming the statements to be true, it appeared to us, for the first time, highly probable that strychnia or nux vomica, in some form, might have been taken by the deceased, either by mistake or through design, and that this had really been the cause of the convulsions and death.

The inquest had been adjourned to the 14th of December, and on that day I attended at Rugeley, for the purpose of giving in evidence the results of our analysis, and, if possible, an opinion on the cause of death.<sup>1</sup>

After hearing read by the coroner, the deposition of Mr.

<sup>1</sup> Dr. Rees was summoned to attend the inquest, but owing to a delay of several hours in the delivery of the electric telegraph message, the cause of which has never been satisfactorily explained, Dr. Rees did not receive the information in sufficient time to attend, or his opinion at the inquest would have corroborated mine

Jones,<sup>1</sup> the medical gentleman who was present at the death of Cook; the evidence of Mr. Bamford, who attended him and prescribed the calomel pills; and of Elizabeth Mills, who alone saw the deceased in the fit on Monday night—respecting his condition on the five days preceding his death, the only conclusion that I felt could be drawn from the medical circumstances was that the deceased *had died in tetanic convulsions, and that these were caused by strychnia*. When this opinion was given, there had been no evidence that the prisoner had purchased strychnia. It was stated to the jury, in reference to the non-detection of this poison in the stomach of Cook, that, unlike arsenic, mercury, antimony, and mineral poisons generally, strychnia might destroy life in a very small dose, and that it was rapidly conveyed into the blood by absorption,<sup>2</sup> so that if a person survived an hour, none might be found in the body. When removed from the stomach by absorption, it had not been detected in other parts of the body. The symptoms, however, which it produced in the living body, were of so peculiar a kind, that these alone, as they were described to have occurred in the case of Mr. Cook, were sufficient to justify a medical opinion that death had been caused by strychnia. The fact that the stomach had been cut open, and that there were no contents sent for examination, except (as it was stated) such as were spread over the surface of some twenty

<sup>1</sup> Mr. Jones had been examined at the first inquest, and had given his evidence in my absence. Mr. Sergeant Shee, in addressing the jury for the defence, asserted that Jones had given his evidence in my hearing, and while I was present; further, that I had suggested questions to bring out evidence that Cook had died from strychnia, and that until I had used the word "tetanus," Jones had not employed it. The whole of these statements are contrary to fact, as the learned counsel might have known, by the various documents before him. When reminded of his error by the learned Attorney-General, he said, "I did not mean to misrepresent anything!" The enthusiasm felt in conducting a defence is apt to carry a man of ardent temperament far out of his depth. There was the same kind of misrepresentation (uncorrected however) respecting the evidence of the sale of strychnia, which the sergeant said had been given at the coroner's inquest before I delivered my opinion. This also was untrue. Roberts, the witness to this fact, was examined some time after me. Misrepresentations of this kind give a false colouring to a defence.

<sup>2</sup> Experiments performed by Dr. Stevenson Macadam, since this trial, have proved that strychnia enters the blood, and is actually, in part at least, discharged from the body in the short period of *nine minutes*. ('Pharmaceutical Journal,' August 1856, p. 124.)

feet of intestines, was already in evidence before them. This was a condition necessarily adverse to the detection of a poison like strychnia, even supposing that the question had simply been whether this poison alone was or was not present. Some of the chemists retained for the defence asserted, at the trial, that this mode of dealing with the stomach and its contents could make no practical difference, although it did not appear that any one of them had ever had a case in his own experience of searching for a variety of mineral and organic poisons, including strychnia, where the stomach had been thus cut and the contents diffused, as alleged, over the whole intestinal surface. When such a case as this has been placed before them, they will be better able to judge of the difficulties, and to express less speculative opinions than those which they gave at this trial. They have endeavoured to teach the profession that carelessness in securing the contents of the stomach, or the stomach itself, in a case of suspected poisoning, is of no importance whatever in reference to chemical results ! It may, however, as any one will find by experience, render it impossible to detect an organic poison with certainty, or, if it be detected, it may render it impossible to say, when the answer to the question may be material, whether the poison found was contained in the stomach or in the fluids which had escaped from the bowels.<sup>1</sup> A quarter of a grain of strychnia diffused through two or three ounces of fluid in a dead stomach may admit of detection, supposing any of the poison to remain therein at the time of death ; but when spread over or diffused through six

<sup>1</sup> Let the reader consider the rashness of such an opinion as this. Assuming that poison had been thus found loosely diffused over the intestines in the jar, this would not have proved that that poison had ever been in the stomach of deceased. It would be difficult, if not impossible, for a person intending to impute poisoning to another, to introduce a solution of strychnia into the stomach when removed and properly secured at the two ends ; but what is there to prevent any maliciously disposed person from secretly pouring a solution of poison into a jar, where it may become diffused over the viscera, and so mixed up with the fluids, as to render it impossible to say whether it might or might not have been in the jar before the viscera were put in, or intentionally added to the viscera subsequently ?

In a case, which occurred to me a few years ago, which was the subject of a trial for murder, the stomach of a child had been carelessly cut open, and the contents lost. The child had been destroyed by a decoction of hemlock. Not the slightest trace of any poison could be found.



or eight pounds of animal matter, including blood and fæces, it is an insult to common sense to assert that the analysis is not rendered infinitely more difficult and far less certain in its results.

At the time of the inquest, however, this question had not the magnitude which it afterwards assumed at the trial. It was considered sufficient to inform the coroner's jury, without entering into minute scientific details, that the circumstances connected with the death of Cook admitted of no other reasonable medical explanation than that it had been caused by one or more doses of strychnia, taken shortly before death. This conclusion was not, in my mind, in any degree weakened by the non-detection of strychnia in the body, because, with respect to this and some other poisons, my opinion was then, and is now, that we may more safely trust to pathology and physiology than to the crude speculations of chemistry.

It turned out upon inquiry, that no antimonial preparation had been prescribed for the deceased by any of those who had attended upon him in a medical capacity since the first attack of sickness at Shrewsbury. How then came the antimony in the tissues of the body, in the contents of the intestines, and in the blood? The quantity found was small, but that was a point of little importance, since, except on the supposition of its having been long resident in the body, a conclusion opposed to its discovery in the fluid contents of the intestines and in the blood, it must have been taken by deceased shortly before his death,—a view supported by the fact that the nausea and sickness from which he had suffered during his illness at Shrewsbury and Rugeley could not be accounted for by any natural disease, while it was in accordance with the operation of small doses of tartar emetic, given to him occasionally by some person about him.<sup>1</sup> At the inquest but few questions

<sup>1</sup> As one instance among many of the ingenious modes in which the defence of Palmer was carried out, I may mention the following incident, communicated to me on good authority. Deceased was first taken violently sick at the Raven Inn, at Shrewsbury, after drinking some brandy and water, which he thought had been "dosed" by the prisoner. Some time after the prisoner's committal, a paragraph appeared in a Shrewsbury paper, to the effect that, as an "extraordinary circumstance," a gentleman had been seized with sickness, like the late Mr. Cook, after drinking brandy and water at the Raven, and it was supposed that the liquors sold at that inn contained something noxious. The paragraph was a pure invention;

were put respecting the antimony. In answer to the coroner, I stated that the antimony found in the body might have been taken by deceased about eight hours before death, and that, from its general diffusion over the body, the conclusion of Dr. Rees and myself was that it had been taken some hours, and perhaps some days, before death. In answer to questions put by the jury, it was stated that the quantity of antimony found in the body would not have been too great to have been given as medicine, supposing that it had been administered by a regular practitioner. Vomiting causes a loss of a great portion of the dose swallowed. It would be scarcely tasted if given in wine or other ordinary liquid articles of food, when the dose was such as to cause nausea and speedy vomiting. The taste would depend on the quantity taken at a dose, and the mode of taking it. A person might unconsciously take a dose sufficient to cause nausea and vomiting. Antimony is not a substance which frequently destroys life, because it is seldom taken or given as a poison. A dose of eighteen or twenty grains might destroy life, but a person might die either from the exhaustion produced as a result of constant and severe vomiting or from the specially poisonous action of the antimonial preparation. Ten grains was the smallest dose which had hitherto been known to destroy life. Antimony should not be given in emetic doses in cases where there was a tendency to apoplexy; it might tend to cause effusion of blood on the brain. It would not be improper to administer it in cases where too much drink had been taken. Convulsions might precede death by antimony if the doses were large.<sup>1</sup>

and, on inquiry, was traced to a person strongly interested in the defence of Palmer. The object of causing such a paragraph to be circulated through the newspapers must be apparent.

<sup>1</sup> I subjoin a copy of my deposition at the inquest on Cook. After reading the report of our analysis, I said—"Therefore the result is, we find antimony in the body, but cannot account for the cause of death. The heart might have been emptied [as the result of] by spasm [either from disease] or poison. Antimony would not [be likely to] cause it [this appearance]. My opinion is that he [deceased] died from tetanus, and that this tetanus was caused by medicine administered and [or] taken shortly before death. I believe that the pills on the Monday night and the Tuesday night contained strychnia. I do not believe that the medicine administered [prescribed] by Dr. Bamford would have produced the effects I have heard [described] to-day. On the Monday night and the Tuesday night after the pills were taken, there was not the slightest indication [of the action]

Considering that in the case of the deceased there was nausea with occasional vomiting, soon after taking certain liquid articles of food, from the time that he was first seized with illness at Shrewsbury until his death, and that there was no natural disease to account for these symptoms,—the discovery of antimony in the viscera, as well as in the blood and contents of the intestines, appeared to us to be quite in accordance with a recent administration of this substance. There was no other explanation which would suit the facts; and although a suggestion was made subsequently, that the antimony might have been in the body of Cook for months and years, and even the whole duration of his life, there was not a shadow of reason to adopt this absurd hypothesis. The various articles of food taken by deceased acted alike in this respect. There had been no symptom of nausea or vomiting prior to the attack at Shrewsbury, and no evidence of deceased having taken antimony at any antecedent period. It would have been, therefore, in our opinion, most improper, when the discovery of antimony in the body was quite consistent with its recent administration, and with the symptoms proved to have existed in the case of the deceased, to have given a speculative opinion that the antimony *might* have been there for any conceivable period. Those who were inclined to adopt this hypothesis of remote deposition, had looked at only one half of the case. Assuming that the metal may remain deposited for several months in the liver, is it probable, after this long period, that it would be found in still larger quantity in a free state in the blood and in the intestines? We, at the same time, gave no opinion respecting its lawful or unlawful administration; and

of morphia on the body. Further than this, we found no mercury in the liver or other parts of the body; and I do not think that mercury or calomel could have been taken on the Monday and Tuesday nights [as well as on the other nights] without our discovering [some] traces of it in the liver. The witness Mills has accurately described the symptoms produced by a small dose of strychnia such as would [might] be caused by pills [containing] strychnia given at half-past ten on Monday night; and the symptoms on the Tuesday night were those which would be produced by a larger dose of strychnia given in the pills taken on that night. There is an absence of any natural cause or disease to account for tetanus. The brain and spinal marrow were [found] healthy. There was no insensibility before death; perfect consciousness, and merely that effect of spasm on the muscular system which a poisonous dose of strychnia would cause."

after hearing the evidence at the inquest, we felt that, medically speaking, death could not be assigned to the antimony found in the body. The circumstantial evidence subsequently given at the trial clearly proved that it had not been lawfully prescribed by any one in medical attendance on the deceased ; and, while it tended to fix the administration of this substance on the prisoner, it fully confirmed the correctness of the opinions given at the inquest.

Much discussion has arisen respecting the object of the prisoner in administering antimony to the deceased. It is not often that two poisons are found in a dead body, or that two different substances are administered at the same time with criminal intention. Viewed by the light of facts connected with the death of the prisoner's wife, as they subsequently transpired at the inquest on Ann Palmer, they showed that one hand had been at work with the same instrument. Although this was a most important fact for public justice, the rules of law shut it out, and the accused had the benefit of the supposition that his wife had died a natural death, and that antimony had not been found in her body. The repeated doses of tartar emetic had, a year previously, succeeded in destroying the wife ; and it may therefore be assumed that, in the case of Cook, the antimony was given in occasional doses, if not with the view of destroying his life, at least for the purpose of inducing an illness of longer or shorter duration, so that the death of the deceased should occasion no surprise by its suddenness. The illness thus produced by the antimony admitted, as occasion might arise, of the administration of some more powerful poison, which would speedily destroy life. It is remarkable that this has been the usual plan pursued by murderers who have acquired a knowledge of medicine. Some have supposed that, by the use of antimony in Cook's case, Palmer had speculated on causing the absorption and disappearance of the strychnia, or on defeating a chemical analysis ; but it is highly probable that the antimony was used by him before he had even resolved upon the use of strychnia. It may be observed, that one effect of tartar emetic is, during the stage of nausea, to promote the absorption and rapid elimination of poisons ; and by its emetic action to cause the expulsion by vomiting, of the residuary portion of unabsorbed

poison contained in the stomach. There was no evidence, however, that deceased had taken any antimonial preparation on Monday, the 19th November, the night on which the first and non-fatal dose of strychnia was administered to him; and all the circumstances tend to show that the antimony was used simply with the view of causing some appearance of illness preceding death.

The coroner's jury did not undertake to solve the question as to the nature of the poison which caused death; they returned a general verdict to the effect, "That the deceased died by poison wilfully administered to him by William Palmer."

It was not until some weeks after the inquest, that I could procure a complete and connected history of the case of Cook, from the date of his first illness at Shrewsbury until his death. This, of course, appears in the evidence of different witnesses at the trial; but the account subjoined, divested of all extraneous details, will be found of great interest in a medico-legal point of view. It will enable the reader to compare the facts as they occurred in this case with the medical inferences drawn by the witnesses for the prosecution and defence.

#### MEDICAL HISTORY OF THE CASE OF JOHN PARSONS COOK, DECEASED.

Tuesday, November 13th.—The deceased, who was 28 years of age, had, generally speaking, enjoyed good health. He was at Shrewsbury races on the 13th, 14th, and 15th November, 1855. Mr. Jones, a medical man, in whose house at Lutterworth deceased had apartments, was with him (by invitation) there on the 13th. Jones dined with him that day. Deceased ate his dinner as usual; and when Jones left him, at 10 p.m., he was in his usual good state of health.

Wednesday, November 14th.—On this day deceased was still at Shrewsbury, in company with William Palmer and other sporting friends. Some brandy and water which he had on Wednesday evening caused violent vomiting. He saw a medical man there, who prescribed a pill and a black draught, and also a sedative mixture. He was seen to his bed-room that night by two friends, who state that they remained with him until 2 o'clock in the morning. He was frequently sick, but not purged. The next morning he was up, and able to attend to business.

Thursday, November, 15th.—Deceased, in company with William Palmer, arrived at Rugeley, from Shrewsbury, at 9:30 p.m. He told the servant, Mills, that he had been ill at Shrewsbury, and was poorly then. He went to bed at 10:30, without taking anything.

Friday, November 16th.—Deceased dined with William Palmer, and came home quite sober.

Saturday, November 17th.—Deceased did not get up to breakfast. He had coffee and was sick immediately after he had swallowed it. He was frequently sick, and ascribed his illness to bile. Palmer sent him some broth during the day; deceased refused to take it, but Palmer insisted upon it. This was subsequently vomited. It appears that some aperient pills were given to him by Palmer in the morning. Mr. Bamford, the surgeon, first saw the deceased, by Palmer's request, at 3 p.m. on this day. He states that he found him suffering from violent vomiting; the stomach so irritable that it would not retain any liquid. He had been in that state for some hours. Pulse 70; skin moist; and the deceased was quite sensible. Deceased was in bed; he complained of no pain in the head, stomach, or bowels. Mr. Bamford prescribed an effervescing saline medicine.

About 7:30 on the evening of Saturday, 17th, Bamford was again requested by Palmer to see Cook. The sickness still continued. Prescribed for him, to be taken at night—Morphiæ Acetatis, gr.  $\frac{1}{2}$ ; Calomelanos, gr.  $\frac{1}{2}$ ; Rhei Pulv., gr. iv; Confect. Aromat., q. s., ft. pil. ij.

Sunday, November 18th.—Between 8 and 9 a.m., Palmer came to witness's house, and they went together to see deceased. He was then constantly sick. Deceased had had two or three hours' sleep in the night, but was sick on awaking. Witness remained ten minutes, and gave him some effervescing medicine in a neutralized state; it was ordered to be repeated every three hours. The liquid vomited in witness's presence was clear as water; no bile or blood, merely mucus and the liquids he had swallowed. [Mills, the servant, states that deceased vomited all Sunday morning. On Sunday evening he had a little barley-water, which remained on his stomach.]

Bamford states, that between 6 and 7 p.m. of Sunday, the 18th, Palmer came to his house, and asked witness to go with him to see deceased. They went: he was then sick, and had been sick all the day; the skin was moist; he had no pain, and was perfectly rational. Bamford further states that, on Saturday morning, deceased had taken, by Palmer's prescription, aperient medicine, and that this had acted both during Saturday and Sunday. The motions were natural; plenty of bile; there was no blood in them. Deceased complained of no pain on pressure; there was no tenderness over the region of the stomach.

It was on this day that Palmer wrote to Jones, the medical friend of deceased, who had parted with him in good health at Shrewsbury, on Tuesday, the 13th, the letter, of which the subjoined is a copy. It does not appear whether Bamford was made cognizant of this letter having been written.

"My dear sir,—Mr. Cook was taken ill at Shrewsbury, and obliged to call in a medical man; since then he has been confined to his bed here, with a very severe bilious attack, combined with *diarrhæa*; and I think it advisable for you to come and see him as soon as possible. Yours faithfully, (Signed), William Palmer, Rugeley, November 18th, 1855 (Sunday).

Monday, November 19th.—Bamford saw deceased between 8 and 9 in the morning; he then complained of sickness, but had not vomited. Prescribed a draught of Sulph. Magnesia, Infusion of Roses, and Tinct. Henbane, every three or four hours. Saw him again between 12 and 1. No nausea, no sickness; said the medicine had agreed with him; had had a basin of coffee, and broth. Bamford saw him again on Monday

evening between 6 and 7. He was then comfortable; had been up. The two morphia and calomel pills were delivered by Bamford to the servant at the inn. These pills were taken, or ordered to be taken, on the Saturday, Sunday, and Monday. There is no evidence that the pills, if taken on Saturday and Sunday, produced any inconvenience or disturbance, or unpleasant symptoms. On Sunday night, Bamford states that deceased wished to have his two pills (asked for them); hence it may be inferred that he had taken them, and derived benefit from them, on the preceding night. This day Palmer went to London, and returned to Rugeley in the evening.

Mills, the servant, states that on Monday, the 19th November, the sickness ceased, and deceased ate some breakfast and dinner, but continued very weak. William Palmer had been with deceased about 9 o'clock p.m. On Monday night, at about a quarter before 12 o'clock, witness was called to Mr. Cook, and saw him sitting up in bed, and beating the bed. He said, "Fetch Mr. Palmer." He had screamed twice. He thought the pills which he said had been given to him by Palmer about 10:30 had caused his illness. *His head was in motion, jerking backwards; his arms were straightened out, and his legs were set quite stiff; the eyes were staring; the head was drawn back; the mouth closed.* He could talk, and he said he should die. Palmer, who had then come over, said, "No, my lad, you won't." Palmer went for some medicine, and gave deceased two pills and some brown heavy-looking liquid—about a wine-glass nearly full. He was sick as soon as he had swallowed the medicine. They looked for the pills in the vessel, but could not find them. Mills remained with deceased until 3 o'clock on the morning of Tuesday, leaving Palmer with him. Deceased did not vomit again while she was there. He asked to have his hands rubbed; they were very stiff, cold, and damp from moisture. Seemed to recover. Deceased had complained of his throat, and thought he was suffering from cold. Palmer remained with him on Tuesday morning after she left. Went to the room about a quarter before 6, and Palmer was not there; he had then left. She did not see Palmer between 3 o'clock and 10 o'clock on Tuesday morning.

Tuesday, November 20th.—Mills saw deceased again at 7 and 8 o'clock; he would not take anything until he had seen Palmer. About 8 o'clock, deceased told witness he thought his illness was caused by the pills. Witness asked, about 10 a.m., whether he would have anything. He again said, "No; not until he had seen Mr. Palmer." At a little before 12 o'clock, deceased sent a message to Palmer, to know whether he might have a cup of coffee. Palmer said that he might, and that he would be with him directly. About this time Palmer appears to have come over. Witness made the coffee and left it with Palmer, who was then alone with Cook. Saw deceased two or three times during the afternoon. At 4 p.m. took him another cup of coffee. Left this also with Palmer, who, in half an hour, gave her a vessel, and said that Cook had vomited the coffee. Saw him several times afterwards that day. Gave him some arrow-root at 8 p.m. This was made in the kitchen. He did not vomit that. She was with him until nearly 10 p.m. He seemed lively and in better spirits. He talked of getting up the next morning; thought he should want the barber to shave him. He asked for some fresh toast and water.

[He appears to have shown no repugnance to drinking fluids, or difficulty in swallowing them.]

Mills had not gone to bed on Tuesday night when the second fit came on. She went for Mr. Palmer. He was over in three minutes (dressed).

Bamford did not see deceased on Tuesday morning. He states that Palmer called on him that morning; said he had been with Mr. Cook; that he was very quiet and comfortable, and *he did not wish him to be disturbed*. (It appears that nothing was said to Bamford about the severe fit on Monday night, or that deceased had ascribed this fit to Bamford's pills; or that he, Palmer, had sat up with deceased from 12 to about 6 o'clock.) Bamford, therefore, did not go near Cook on Tuesday until about 7 or 8 o'clock in the evening, when Palmer again called for him. In the mean time, Jones arrived. Jones, who had left deceased in good health at Shrewsbury on the 13th, arrived at Rugeley on the afternoon of the 20th, at 3 o'clock, and found deceased in bed. In reply to questions put by him, Cook said he was very comfortable, but very weak, and was not allowed to say much. He said he had been very ill. Mr. Palmer came in soon afterwards. Jones then examined Cook in the presence of Palmer; found his pulse soft and natural, and his *tongue clean*. Remarked to Palmer that it was hardly the tongue of a patient suffering from *bilious diarrhœa*, when Palmer said, "You should have seen his tongue before." [Bamford does not seem to have diagnosed bilious diarrhœa at any time; and Palmer had himself given active purgative medicines in pills on Saturday morning before Bamford was called to see deceased.] Jones saw him every half hour during the afternoon, sitting with him, and going down stairs occasionally. [It does not appear that Palmer then informed Jones of the severe attack which deceased had had on Monday night, and which had led to his being called up and sitting with deceased for about five hours during the night.]

Mr. Bamford came with Mr. Palmer about 7 o'clock in the evening, and Mr. Bamford expressed an opinion that deceased was then going on *satisfactorily*.

Bamford says he put down the bedclothes and examined deceased particularly about the bowels and stomach, in the presence of Jones and Palmer. Deceased was then irritable, and appeared uncomfortable in his mind—distressed; turned his face from witness, who was on one side of the bed, while Jones and Palmer were on the other. The pulse was between 80 and 90, "full, irritable, firm, and trembling." Deceased said, I will have no more pills to night. He had asked for them on the previous night. It was agreed in consultation, however, that the pills should be given, and Palmer said to Bamford, I would rather you would make the pills again to-night. *He made them up in Palmer's presence, wrote a direction, at Palmer's request ("Night pills"), and delivered them to Palmer, who took them away with him.* Bamford did not again see the deceased alive.

Jones states that at the consultation it was suggested by Palmer that Cook should not know what the pills contained, as he strongly objected to them on the previous night, because they had made him ill.

Palmer came with the pills about a quarter past 11 o'clock. He produced two pills, and gave them to deceased in Jones's presence, calling Jones's attention to Bamford's handwriting on the box. Almost immediately after swallowing the pills, deceased vomited. It was a momentary act. The pills could not be seen in the chamber-vessel. Deceased then lay down, and was quiet. He dreaded an attack like that of of the previous night, which he described as a kind of fit. Witness went to bed in the same room, at a few minutes before 12. At that time deceased appeared as comfortable as usual. At about ten minutes past 12, Jones was suddenly roused by deceased, who had jumped up in bed, saying, I am going to be ill, ring the bell for Mr. Palmer. He asked witness to rub his neck. Soon after Palmer came (*i. e.*, in



(about two or three minutes). He brought with him two pills, which he gave to the deceased. He said they contained *ammonia*. Immediately after taking these pills, the deceased screamed loudly, and threw himself back on the bed, in convulsions. He asked to be raised up, saying, "I shall be suffocated." They endeavoured to raise him, but he was so stiffened out with spasms, that it was impossible to do so. When he found we could not raise him, the deceased said, "Turn me over." Jones then says, "I turned him on his right side, listened to the action of his heart, which gradually ceased, and in a few minutes he died. I have never seen symptoms (convulsions) so strong before. They were symptoms of *tetanus*; every muscle in the body was stiffened. When the deceased asked to have his neck rubbed, which was in about three minutes after I was first roused, I found that his head and neck were affected with spasms. His head was thrown back, his hands were clenched, and his arms were in a state of rigidity. His jaw was fixed and closed. His body was stretched out, and resting on his head and heels (opisthotonos)."

[It thus appears from Jones's statement, that at 12 o'clock, and until about ten minutes past 12, the deceased Cook was in a comfortable state. In three minutes, *i. e.* at 12:13, the spasms in the head and neck were strongly developed. After this, Palmer came, and the deceased asked him for the remedy he had taken the preceding night. The remedy, *i. e.* the ammonia pills, were taken in about ten minutes, making the time 12:20. Deceased lived six minutes after swallowing the two pills brought by Palmer, making the whole duration of the case from sixteen to twenty minutes, death having taken place at 12:26, or thereabouts.]

*Inspection.*—This was made six days after death, in the presence of Palmer and others. The viscera were universally in a sound state. The membranes of the brain were a little congested (*dura mater*). The heart empty; the blood generally fluid. No appearances in any part to account for death. The body remained in rigid spasm after death, and was so found in the extremities on exhumation, more than two months after death.

*Analysis.*—The only discovery made was, that absorbed *antimony* had been deposited in, and was contained in the tissues of the stomach, intestines, liver, kidneys, spleen, and blood.

The cause of death, on the one hand, is assigned to strychnia given in the pills taken at a quarter past 11 on Tuesday night; on the other, to some natural disease—idiopathic tetanus or hysteria.

The deceased had two attacks, one on Monday night, and the second, or fatal one, on Tuesday. They were precisely similar in their character, but that of Tuesday was more severe. The attacks came on about *an hour* after some pills had been taken by the deceased on *each* night. He had perfectly recovered on the Tuesday morning, and remained in a very comfortable state during the whole of that day, and in fact until, without any assignable cause, the symptoms suddenly appeared, and killed him *under tetanus and opisthotonos of the most formidable kind, in sixteen, or at the furthest, twenty minutes* from the commencement of the attack.

Can it be reconciled with idiopathic tetanus, or with any known disease of the nervous system, that it should thus appear, without any assignable natural cause, on two successive nights, that there should be a complete intermission of the tetanus for nearly twenty-four hours, and that it should then again burst forth and prove fatal in the short period mentioned?

The fate of the prisoner at the subsequent trial depended on the answer to this question. But there was another question closely connected with the above, on which great difference of opinion was likely to arise; namely, whether any description of symptoms or medical circumstances could justify a medical witness in assigning death to poison when he was unable, by analysis, to reproduce from the dead body, in a visible and tangible form, some portion of the poison alleged to have caused death.

As the supposed natural disease, if any, would probably have affected the spinal marrow—and the upper part only of this organ had been examined and found healthy—it was considered desirable for the ends of justice, that a more minute examination should be made. The body was exhumed about two months after death (January 25th, 1856), and it was then ascertained that the spinal marrow and the vertebral canal were quite healthy,—that there was no change or appearance in these parts, which could be assigned to disease during life. There was nothing, in short, in the condition of this organ which would account for the sudden attack of tetanus and death. Two medical gentlemen attended the inspection on the part of the prisoner; and they had full liberty to take away any, or the whole of the body, for the purpose of verifying the accuracy of the analysis already made, as to the presence or absence of antimony and strychnia. Only a small part of the spinal marrow and its membranes was, however, taken by them, with a view to a pathological examination, and not to a chemical analysis. This is a significant fact, and it tends to throw an important light on the chemical portion of the defence, and on the *bona fides* with which the scientific question of the presence or absence of strychnia in the body of Cook, was subsequently treated.

To my apprehension, the symptoms under which Cook died, as described at the inquest, were quite irreconcilable with any known form of disease. This judgment was not formed by taking fractional parts of the case, or adopting the practice pursued by some of the medical witnesses for the defence at the trial, namely, of making comparisons of individual symptoms in different cases. The case was viewed as a whole;

and as such, it stood out clearly and distinctly as a case of poisoning by strychnia.

The symptoms indicative of tetanus as a result of strychnia in a fatal dose are—trembling of the muscles, shaking of the body, stiffness of the back of the neck, general uneasiness, and a feeling of suffocation. The body and extremities are suddenly stiffened by muscular spasm; the hands are clenched; the toes are incurvated, the soles of the feet becoming hollow; the body then assumes a bow-like form; it rests on the head and heels, and the back is arched; the muscles of the abdomen are hard. During the paroxysm there is severe pain, leading to shrieks and cries, from the almost universal cramp; the face is dusky or livid, the lips are livid; the mind is clear; there is consciousness. Remissions of these symptoms occur at short intervals; but, after a succession of the fits, and sooner or later in proportion to their intensity and duration, the person dies either from asphyxia, or as a result of exhaustion of nervous power, by reason of their frequency and severity.

I here transcribe from my notes, written long before the trial, a summary of the reasons on which my opinion of the cause of death in Cook's case was based:—

1. *Reasons for the Opinion given at the Inquest, that Cook died from the effects of Strychnia.*

The symptoms were undoubtedly such as a dose of this poison would cause. They came on very suddenly about an hour after some pills had been swallowed. These pills should have contained morphia and calomel, and have produced a sedative effect on the body—calming any excitement and producing sleep. On the Saturday and the Sunday night the pills appear to have had a good effect; at least, no injurious symptoms were produced by them—no convulsions, no spasms. If Bamford, therefore, prepared the pills from the same bottles, and according to the same prescription, on the four nights which preceded death, he could not have made a mistake, and put strychnia for morphia, apart from other facts stated by him at the inquest, showing that he could not have mistaken his bottle of strychnia for that containing morphia.

On the Monday and Tuesday nights only do these pills act differently. Instead of the calmative effects of morphia, there are the violent symptoms of strychnia. The deceased himself ascribed his symptoms on Monday night to the pills which he had taken, and refused to take any more.

While there is no indication of such effects on the living body as the morphia pills ought to have produced, and had apparently produced on the two previous nights, there is no trace of mercury to be found in the liver after death. Had the four doses of pills containing calomel been swallowed on the four successive nights, it is in the highest degree probable that some trace of mercury would have been found in

the liver. I state this, on a remarkable case which occurred to me some years ago, in which some months after death I detected mercury in the liver of a person who had taken only a few grains of blue pill shortly before he died. It would not be proper, however, to rely greatly on this point. It might fairly be open to doubt and objection. I would rather base an opinion on the symptoms which actually followed in the case of Cook, being so entirely different from those which ought to have followed, supposing the pills to have contained morphia and calomel, and not strychnia.

Before leaving this part of the case it should be stated, that, subsequently to the inquest, Dr. Rees and I analysed the contents of a pill-box found in prisoner's room, and detected therein a preparation of mercury (calomel?), and a substance resembling morphia in its chemical reactions. The substance was broken up, and did not correspond in weight to Bamford's pills.

*Time of Occurrence of Symptoms.*—The symptoms came on, on each night, *i. e.* (Monday and Tuesday), about an hour after the pills had been taken. This is quite in accordance with the operation of strychnia. When the strychnia is dissolved, the symptoms may come on earlier. The time at which they appear after the poison has been taken, may vary according to the state of constitution, and many other circumstances. When the poison has been absorbed in sufficient quantity, they begin to show themselves, and the rapidity of absorption probably varies in different persons and in different states of the body. The pills taken by deceased on the Monday night could not have contained sufficient strychnia to destroy life: but the symptoms were severe, and lasted some time. From one quarter to one half of a grain might account for the effects on Monday night; and three quarters of a grain to a grain might account for the symptoms and death on Tuesday night.

The effects produced on Monday night would enable a person acquainted with the properties of strychnia to adjust the fatal dose with tolerable accuracy on the Tuesday night.

## 2. *Reasons for the Opinion that Cook did not die from any form of Tetanus as a result of disease.*

There is no doubt that the actual cause of death was tetanus in a very severe form. The question is, therefore, what was the cause of the tetanus?

Tetanus, which implies a general spasm, or cramp of all the muscles of the body that are usually under the power of the will, may arise from,—1, poison; 2, wounds (lacerations, severe bruises) (traumatic); 3, exposure to cold and wet (idiopathic).

As to *poisons*—arsenic, antimony, and other irritant poisons, may occasionally produce tetanic spasms of the muscles, but then there are always other symptoms which precede or follow, of a totally different kind.

Strychnia is the only poison (with the exception of brucia) which produces tetanus in a pure and unmixed form, and this was tetanus in its pure and unmixed condition.

In Cook's case there was no wound or personal injury; hence it was not a case of what is called traumatic tetanus. Idiopathic tetanus is comparatively a rare disease, not very often fatal, and then only after a long period.

There was no exposure of the deceased to wet or cold. He remained from Wednesday night until the following Monday without any symptom indicative of tetanus, until an hour after he had taken the pills on Monday night. Bamford saw

nothing to indicate tetanus on the Saturday and Sunday. There is therefore (apart from the pills) no cause to account for an attack of tetanus in the case of Cook; and being a case unexampled in the severity of the symptoms, and its rapidly fatal termination, on the assumption of its being idiopathic tetanus, there ought to be some apparent cause for its origin. There was no proportionate excitement to account for it, and the morphia and calomel prescribed by Bamford would have tended to allay such, had it existed.

## DIFFERENCES.

*Idiopathic Tetanus from Exposure to Cold and Wet.*

1. Symptoms have no connection with any liquid or solid swallowed.

2. Symptoms commence slowly, and progress slowly; difficulty of swallowing stiffness of jaws, of neck; after some time the body, the legs, lastly the arms; hands not commonly affected.

3. Opisthotonos, or body bent backwards in the form of a bow, resting on the head and heels; does not come on until after many hours or days from the attack.

4. Paroxysms, or fits of spasm, may be severe, and the person may die from exhaustion. Patient commonly recovers after some days or weeks.

5. In idiopathic or traumatic tetanus there is no *intermission* in the symptoms, merely a remission of the paroxysms. The patient is always under the influence of the morbid cause, which remains until he dies or recovers.

*Tetanus from Strychnia.*

1. Some solid or liquid taken within about two hours or less of commencement of symptoms.

2. Symptoms commence suddenly with great violence. Nearly all the voluntary muscles of the body are simultaneously affected. Arms and hands spasmodically clenched at the same time as body and legs. Jaw not primarily affected, not always fixed.

[N.B. Cook was able to swallow and speak within ten minutes of his death. No case of idiopathic tetanus, so far as I can find, has presented this condition, the jaw being generally the first part to become fixed.]

3. Opisthotonos, a very early symptom, in a few minutes commonly.

4. When symptoms are once clearly established, they progress to death or recovery. They occupy only minutes. In from ten minutes to two hours after commencement, the person dies or recovers, according to the severity of the paroxysms, and strength of his constitution.

5. In tetanus from strychnia, if the dose should not be sufficient to prove fatal, the effects pass off; patient recovers; there is a complete intermission in the symptoms.

[N.B. This is a remarkable feature in Cook's case; since, on the Tuesday, he had quite recovered from the attack on the Monday night.]

The differences here assigned, show that in the case of Cook the tetanus was produced by something administered, and not by any ordinary cause.

It may be objected to the medical opinion, that Cook died from an attack of *hysteria* simulating tetanus.

#### DIFFERENCES.

##### *Hysteria.*

1. Connected with a peculiar constitution, chiefly seen in females.

2. Patient subject to previous attacks or fits.

The spasms may be tetanic in hysteria, but there are more commonly convulsive motions of the limbs, alternating with stiffness or rigidity, generally a loss of consciousness, and other symptoms of hysteria.

3. Paroxysm not fatal; patient speedily recovers.

##### *Tetanus of Strychnia.*

1. Not connected with any peculiarity of constitution.

2. Cook had not been subject to such attacks at any period; never had had any kind of fit before this. A healthy active young man in the prime of life, given to out-door pursuits.

If occasionally subject to excitement, the effects (if any) would supervene immediately, and not be postponed for several days. There was no cause of hysterical excitement on Monday and Tuesday night.

3. He speedily dies in a paroxysm.

No instance, so far as I know, is recorded, in which a young and healthy man in the prime of life, has been at once (suddenly) seized with a fit of hysteria, presenting only the features of tetanus from poison in its most severe form, and like it, proving fatal in twenty minutes from the first attack or commencement of symptoms.

##### *Post-mortem Appearances.*

There is nothing of a peculiar character in these appearances, whether the tetanus depend on disease or on strychnia.

In Cook's case, the principal appearances were,—1, fluidity of the blood; 2, emptiness of the heart; 3, some congestion of the dura mater, or outer membrane of the brain.

All that can be said is, that these appearances are consistent with death from strychnia; and further, they fail to show in any part of the body any *natural cause* for this violent and sudden death. The spinal marrow was subsequently found healthy.

There is not much probability that a case of tetanus from disease could be mistaken for tetanus from strychnia-poisoning. The risk is almost entirely the other way. Dr. Watson justly observes, our eyes should be open to the possibility of a case of poisoning by some of the preparations of strychnia, being palmed upon us for a case of natural

disease.<sup>1</sup> This form of poisoning, he remarks, may be easily mistaken for tetanus. The rapid progress of the symptoms, and speedy death, have always been regarded as strong indications of poisoning. There is a case of traumatic tetanus, as it is called, quoted by one author from another, on the authority of Prof. Robinson, of Edinburgh, who died about the end of the last century, which it is said proved fatal in a quarter of an hour. The anecdote is this :—The professor was once at table, when a negro servant lacerated his thumb by the fracture of a china dish. He was seized with convulsions *almost instantly*, and died with tetanic symptoms in a *quarter of an hour*. It is highly probable, as Dr. Watson observes, that assuming the statement to be true, the man died rather of fright than of traumatic tetanus. The average period of the access of tetanus from a wound is from the fourth to the fourteenth day. In the above case, the man was a negro, and negroes have been observed to be particularly liable to tetanus from slight causes. There was a lacerated wound also: hence, admitting the authenticity of the case, it could not be compared with that of Cook, for there was no injury to his body which could account for the occurrence of tetanus. An attempt was made to introduce this case, but no stress was laid upon it. I subjoin a statement of the most rapidly fatal cases of tetanus, as it was prepared for evidence at the trial, also a statement of some cases in which tetanus followed and proved fatal after ulcers. It was quite clear, from the evidence of Dr. Savage and others at the trial of Palmer, that Cook had no ulcers to which tetanus could be reasonably referred, and if he had had ulcers at the time of death, the fact that he was seized with tetanus and died in twenty minutes would have been quite inconsistent with all experience on the fatality of tetanus from such a cause. The supposed ulcers could not have been a cause of tetanus on the Monday night; and after this, there was complete recovery for twenty hours before the second and fatal attack. Nevertheless much was said about ulcers, and there was obviously an intention on the part of the defence to present this to the jury as a satisfactory explanation of the tetanus under which deceased sank.

In Case 102 in a table compiled by Mr. Curling, tetanus

<sup>1</sup> Practice of Physic, i, 558.

ended fatally in twelve hours. In Dr. Laurie's table of 41 fatal cases, in the Glasgow Infirmary, the shortest period in which the disease destroyed life was twelve hours after amputation of the thigh. In a boy, whose case is detailed in Mr. Curling's work, the disease proved fatal in sixteen hours. The above cases were traumatic.<sup>1</sup>

In Dr. Laurie's table of 50 traumatic cases, in the Glasgow Infirmary, and in his table of 171 published cases of traumatic tetanus, one in the former, and two in the latter, occurred after *ulcers*. Mr. Hutchinson, 'Medical Times, vol. xxix.' gives the case of a man (Case 31) having an ulcer on the leg, of unknown duration, in which death occurred after a single day, with symptoms of tetanus. Fournier Pescay very accurately describes, in the *Art. Tetanus*, in the Dictionnaire de Médecine (1821), the case of a soldier, who wounded the little finger with an axe: he was seized with tetanus in three hours, and died in fourteen hours after the accident.

In March, 1856, a case of tetanus, arising from ulcers, was brought into the London Hospital. This proved fatal in from eight to ten hours. The details were given in evidence at the trial of Palmer by Mr. Ross. A man, *æt.* 37, having chronic indurated ulcers, two at the back of the right elbow, near the ulnar nerve, and one on the left, was brought into the hospital at half past seven in the evening. He was then breathing quickly, the jaws were closed; he was unable to swallow; the muscles of the abdomen and back were somewhat tense. In ten minutes he had a second paroxysm (having previously had one in the receiving room), with opisthotonos, which lasted about one minute; he was then quiet for a few minutes; he had then another fit, in which he died, having been in the hospital only half an hour. On cross examination, it was elicited that the symptoms in the jaws had come on about *nine hours before his admission*, and that there had been convulsive paroxysms all the afternoon. Mr. Ross had no doubt that the man died from tetanus, and that the cause of the tetanus was referable to the indurated chronic ulcers described. Thus, then, this was simply proved to be a fatal case of tetanus from ulcers. The stomach of deceased was examined for strychnia, but of course none was found. Was there, indeed, anything about the case that could have led to the

<sup>1</sup> See Appendix to this paper, p. 136.



supposition that the symptoms were caused by strychnia? Was the treatment, on admission, such as is immediately resorted to in cases of strychnia poisoning? In the first place there was no evidence that the man had swallowed anything or had been able to swallow anything since the time at which he had had his dinner, *i. e.* about eight or nine hours before his admission! There was no reason to suspect an intention to commit suicide; or that his wife, who came with him to the hospital, had intended to poison him. Again, the convulsive symptoms commenced and progressed very slowly, so that eight hours had elapsed before they had acquired great severity, and the man lived, from the date of their commencement, the long period of nine hours, or thereabouts! There was a visible and satisfactory cause for this tetanus in the old chronic indurated ulcers under which the deceased had been suffering for twelve years. The symptoms began, as in most cases of traumatic tetanus, with stiffness, followed by spasm and fixedness of the jaws; the abdomen, back, and extremities being only long afterwards affected; there was inability to swallow; and although the arms and legs were convulsively affected in the paroxysms preceding death, it does not appear that there was any clenching of the hands, or incurvation of the feet. Apart from the absence of all moral evidence, there was not a single medical feature in the commencement, progress, and duration of the symptoms in this case, by which it could be confounded with a case of strychnia-poisoning. Although more rapidly fatal than traumatic tetanus is usually observed to be, it would have been a case of uncommon duration for one of strychnia-poisoning. In tetanus from disease, the case is reckoned by hours or days, in tetanus from strychnia, by minutes. There was not one point of resemblance between it and that of Cook.<sup>1</sup>

<sup>1</sup> The subjoined paragraph, in reference to this case, appeared some time before the trial of Palmer, in the Birmingham and Staffordshire papers, and was afterwards copied into some London papers. This may furnish a key to its being produced in evidence. It would of course find circulation among the class of men from whom jurors are selected.

“An extraordinary death from tetanus has taken place at the London Hospital. Edward White, aged thirty-seven, was brought in with symptoms as of poisoning by strychnine; his wife said he had been ill some days, and he died a few hours after admission. An inquest was held, at which Dr. Letheby stated that he made an analysis of the deceased's stomach and its contents. He searched for strychnine in

I willingly admit that it would have saved Dr. Rees and myself from much obloquy and vituperation, had we assigned tetanus, in Cook's case, not to strychnia, but to the idiopathic disease from cold taken on the race-course at Shrewsbury, to angina pectoris, or to a sudden attack of "epilepsy with tetanic complications," depending upon some unrecognisable disease of the spinal marrow,—ignoring at the same time the presence of antimony in the body, and referring the nausea and sickness from which deceased had suffered to chronic irritation of the stomach, brought on by intemperance. Nothing would have been more easy than to have given an opinion, such as that which was given in the case of Miss Abercromby, that cold and hysteria would account for tetanus and sudden death. In short, from the circumstances so favorable to the accused under which this inquiry was conducted, there would have been no difficulty whatever in entirely withdrawing from public notice Palmer's crime.<sup>1</sup> The reward for acting with decision and firmness on this occasion has not been such as I think will induce professional men to follow my example in any similar case

consequence of the symptoms, but found no poison at all. Since so much had been said about strychnine, witness had made numerous experiments with it, and could now detect the hundredth part of a grain if it had been present. The cause of death was tetanus. It was an extraordinary case, and he knew of no other on record. A verdict was found "the deceased died from tetanus, but how caused there was not sufficient evidence to show."

<sup>1</sup> The emptiness of the heart was, in my absence, dwelt upon by the coroner, in his final address to the jury, as a condition inconsistent with the opinion which I had given of the cause of death: because a case had been described by me, in my work on 'Medical Jurisprudence,' in which the heart and lungs were found gorged with blood. I had already, however, answered the questions put to me on this point, and had quoted to the jury another case of poisoning by strychnia, in which the heart was found empty. The coroner is reported to have impressed upon the jury, that Mr. Bamford, who was present at the death of the deceased [an error] thought that he had died of apoplexy, and "even after the brain had been cut open," he was of the same opinion! With a jury of more pliant character, this would have had the effect desired by Palmer, in his letter addressed to the coroner shortly before.—"I hope the verdict, to-morrow, will be, that he died of natural causes, and thus end it." If the coroner believed that the published views of a witness were inconsistent with his evidence given at the inquest, it was his duty to point out this inconsistency to the witness when present, and not reserve his comments for a time when the witness was absent. It would then be out of the power of the witness to explain the differences, or to satisfy the jury that such inconsistency did not exist.

hereafter. There is, however, only one plain duty to be performed on such occasions, however painful it may be. Whatever may be the consequences to the medical jurist, although he may stand alone, and have the attacks of the public and medical press directed against him, the safety of society and the demands of public justice, require that an opinion formed deliberately after a careful consideration of all the medical circumstances, should be firmly and decidedly expressed. After all, it must be remembered, the medical opinion refers to the cause of death, rather than to the criminal. The medical facts may admit of only one reasonable interpretation, namely, that the person died of poison; it must be the conduct of another, his motives and his actions, irreconcilable with innocence, which will ultimately fix the perpetration of the crime upon him. The medical opinion of the cause of death in Palmer's case, had no relation to the prisoner's conduct—in his assigning a false cause for the illness; in his endeavouring to procure a hurried interment of the body; in his interfering at the post-mortem examination, and cutting into the jar containing the viscera; in his attempting to bribe a post-boy to overturn the carriage and break the jar; in opening, or causing to be opened, letters having relation to the results of the analysis; in writing to his friend the coroner, expressing his desire that the inquest might be brought to a close with a verdict of death from natural causes, &c. I might add to these circumstances the possession of strychnia, without any assignable or reasonable motive for procuring this formidable poison, by gift as well as purchase, on two different occasions within twenty-four hours of Cook's death. It was not the medical opinion respecting the cause of death, or, as I have elsewhere remarked, any medical theory regarding the operation of strychnia, which led to the conviction of the prisoner at the trial, so much as these portions of his conduct, for which the subtle and overstrained arguments of his counsel could afford no reasonable or satisfactory explanation. They were, in fact, left without an answer.

The view propounded by Mr. Sergeant Shee, however, was that the medical opinion of the cause of death given at the inquest, was based, not upon the symptoms, but on the knowledge that strychnia had been purchased by the prisoner. In

the kind of defence set up on the occasion of this trial, at which every circumstance, medical and moral, adduced by the prosecution, tended to establish the guilt of the accused, it would have been vain to expect a charitable, or even a reasonable view of the motives for medical opinions. It happened, however, unfortunately for this part of the defence, that the symptoms suffered by Cook on the Monday night, were equally referred to strychnia, although there was then no proof of the purchase of the poison by the prisoner until midday on the Tuesday, the day on which the deceased died. Besides, as I have elsewhere stated, the fact of the purchase on the Tuesday was communicated to Dr. Rees and myself, before we had even sent our report; and that report, as it is well known, did not assign death to strychnia or to any other cause. There was no evidence of the possession of strychnia by the prisoner on Monday night, until the day before the trial, *i. e.* five months after the opinion had been given at the inquest, that the symptoms on that night were distinctly due to strychnia. On this point, therefore, the defence utterly failed.<sup>1</sup>

I need not remark that the opinion given at this inquest, regarding the cause of death, was fully confirmed at the subsequent trial, by the evidence of Sir Benjamin Brodie, Dr. Todd, and others. With the views so clearly expressed by these witnesses on the differences between the symptoms of tetanus as a disease, and those caused by strychnia, and their concurrence in the opinion, that while the symptoms manifested by Cook on the

<sup>1</sup> This part of the defence, involved a striking inconsistency:—the medical opinion that the symptoms were caused by strychnia on the Tuesday night was alleged to have depended on a knowledge that the poison had been purchased on that day; but the medical opinion, that the symptoms on the Monday night had arisen from precisely the same cause, had been formed and given without any knowledge of the possession or purchase of the poison, and maintained for five months in the absence of any proof to that effect. Some have thought that the procuring of three grains of strychnia, at Rugely, on Monday night, was an act which would not have been done by a guilty man. The poison, however, was *not purchased*, but procured as a gift from the house of a person where drugs and chemicals were not sold. As this was not a retail shop, no inquiry was made there, and none knew, or could possibly know, that poison had been given, except the giver and the receiver. The gift of poison at this date was shortly afterwards communicated by the witness Newton, to Cheshire, the postmaster. Newton's evidence of the gift was thus corroborated. Had strychnia been procured at a retail chemist's in London, or elsewhere, the purchase would have been soon traced by the police. Therefore this mode of procuring the poison on Monday was well adapted to conceal the transaction.

Monday and Tuesday nights were quite consistent with the effects of strychnia, they were not in accordance with those of any known form of disease, I consider it quite unnecessary to discuss this question further. The chief witnesses for the defence were compelled to admit that the symptoms were such as strychnia might produce, and the reason probably why Sergeant Shee, did not call—Mr. Lawrence, surgeon of St. Bartholomew's Hospital, who attended in Court, for the defence; Dr. Williams, Professor of Materia Medica in the Royal College of Surgeons in Ireland, and surgeon for eighteen years to the city of Dublin Hospital; Dr. Nicholas Parker, of the London Hospital (both of whom, he promised, in his address to the jury, to place in the witness-box), Dr. Marshall Hall, Dr. Copland, Dr. Carpenter and others—was that they would in all probability have confirmed substantially the evidence of the witnesses for the prosecution, and have given an opinion that the symptoms of Cook were those of strychnia, and not such as they had ever witnessed in tetanus from disease. With regard to Drs. Copland and Watson, they were easily accessible as witnesses if they could have testified in favour of the prisoner; but the learned sergeant preferred relying upon selected extracts from their published works! The other witnesses who were examined for the defence, did not agree among themselves respecting the cause of death;—thus the jury were left to make their selection from some convulsive disease, modified epilepsy, hysteria, arachnitis, epilepsy with tetanic complications, and angina pectoris; one witness professed that he was unable to assign any cause for the symptoms, either natural disease or poison; but he thought that “some peculiarity of the spinal cord, unrecognisable except the examination be made immediately after death, might produce symptoms like these;” a very convenient hypothesis for covering any kind of death from poison affecting the nervous system, when from circumstances the poison cannot be discovered in the dead body. It simply implies that a *recent* examination of the spinal cord sets all questions at rest respecting the pathological seat of convulsive diseases; and that hydrophobia, hysteria, tetanus, and epilepsy, are at once indicated by the inspection of this organ, while the body is recent!

One of the strongly contested questions at the trial—involving many of the points just considered—was this :

WERE THE SYMPTOMS AND APPEARANCES IN THE CASE OF COOK CAUSED BY STRYCHNIA, OR MIGHT THEY HAVE ARISEN FROM SOME NATURAL DISEASE ?

On this question, as it has been already remarked, the evidence of all the witnesses for the prosecution was clear, distinct, and conclusive ; and the three cases of death from strychnia—namely, those of Agnes French, Mrs. Sergison Smyth, and Mrs. Dove—supplied all that was wanted to make out a complete history of poisoning by strychnia in the case of John Parsons Cook. Certain objections to this conclusion were taken by the witnesses for the defence, on the ground that some of the symptoms in Cook's case were inconsistent with poisoning by strychnia ; and it may now be desirable to consider how far these objections are well grounded. The dissentient views expressed at the trial were chiefly derived from what had been witnessed of the effects of strychnia on animals. One witness thought that the very *sudden accession* of the symptoms was greater in the case of Mr. Cook than in strychnia-poisoning. According to my experiments, and those of others, the symptoms in animals generally come on suddenly, after some slight uneasiness. But observations on the human subject are more to the purpose.

In the case of Agnes French, detailed at the trial, the symptoms came on suddenly, and the patient was found in violent convulsions. (See No. 4 in the table of fatal cases at p. 79.) In the case of Mrs. Smyth (No. 10 in the table, p. 82) they also came on suddenly, and the ringing of a bell, which was considered to be a difficulty in Cook's case (although this took place on the Monday night, and not on the Tuesday), was also an act performed by Mrs. Smyth, after she had taken a fatal dose of strychnia. The *sitting up* and *beating of the bed* (the *malleatio*<sup>1</sup> of Sergeant Shee), and the screaming, observed

<sup>1</sup> *Malleatio*. This learned word was applied to that part of the evidence of Elizabeth Mills, in which she states, she saw Cook "sitting up in bed, and beating the bed." Hooper, in his dictionary, tells us, that it is a form of chorea, in which the person has a convulsive action of one or both hands, which strike the *knee* like

in Cook's case, as well as the power of having his neck rubbed without causing spasms, are quite consistent with poisoning by strychnia. The first two acts are those to which a man complaining of a feeling of suffocation from any cause would be likely to resort, and may, like the screaming, precede the access of spasms. They were, in fact, felt by the patient himself to be the forerunner of an attack of spasms, and described by him as such. As to the screaming and tolerance of the *rubbing* of the body, Mrs. Smyth's case (No. 10, p. 82) affords an example of this. A case published by Dr. Lawrie and Dr. Cowan shows that, in poisoning by strychnia, this rubbing may not only be borne, but give relief.<sup>1</sup> It is absurd to describe Mrs. Smyth's as an exceptional case, merely because great sensitiveness of the skin is generally manifested in the poisoning of *animals* by strychnia. The record of cases in the human subject does not bear out the view that human beings are similarly affected, and the statement that animals are *always* so sensitive that they will not bear rubbing without the production of tetanic spasm is not in accordance with well-observed facts. Drs. Lawrie and Cowan justly remark that it is "the sudden first impression on touch, and not the *continuous pressure*, which causes the shock and the renewal of the spasm,—a circumstance which appears to have been entirely overlooked by the witnesses at Palmer's trial." \* \* \* "We

have uniformly found," they observe, "that rubbing any part of the surface in dogs does not cause spasm; and that, so far as we could judge, rubbing or scratching their necks was agreeable to them." As the Attorney-General remarked, the dogs and rabbits which were the subject of experiment could not ask to have their necks rubbed or manifest the "malleatio;" and, although much reliance was placed on their acute sensitiveness to slight touches, under the operation of strychnia, not

a hammer. A more probable interpretation is, that Cook was in a distressed state from a feeling of oppression, and a sense of suffocation, and that he was simply doing what patients frequently do under the same circumstances, throwing his arms about on the bed. Why such a learned term should be applied to so simple and natural an act as this, it is difficult to understand, but for the fact, that it was thereby raised into the dignity of a special symptom, by which Cook's case was to be distinguished from other cases of strychnia-poisoning. The ringing of a bell might as justly have been transformed into a symptom, under the name of "tintinnaculatio."

<sup>1</sup> 'Glasgow Medical Journal,' part xiv, July, 1856.

one witness appears to have tried the effect of rubbing or stretching the muscles, or it would probably have been found that it could have been borne, as in the cases of the animals thus treated by Drs. Lawrie and Cowan.

A question, however, here suggests itself. Does it follow that the acute sensitiveness observed in animals should also be observed in man? May not the poison affect man and animals differently in this respect? Trismus is a common and early symptom in animals. It is not usually observed in human beings until after the paroxysms of convulsions have set in, and there may be equally a difference with respect to sensibility. Devergie observes that all poisons which affect sensibility, whether local or general, have not the same influence on man and animals, because it is impossible to establish the least comparison between the sensibility of the dog and that of a human being.<sup>1</sup> According to Pereira, the exaltation of sensibility in man is rather the effect of a succession of large doses used medicinally, and acting perhaps with accumulative force, than of one fatal dose. It is to be remarked that the observations of the witnesses, so far as they referred to the human subject, were confined to cases in which strychnia had been used medicinally. They had not seen one case of acute poisoning by strychnia like that of Mrs. Smyth or Mr. Cook.

The next important objection is based on the act of *vomiting*. Mr. Nunneley is asked by Mr. Sergeant Shee—"Is there any medical reason that occurs to you why the patient (poisoned by strychnia) should *not* vomit? *A.* I apprehend, where there is so much spasm, there is an inability to vomit: in cases related where attempts to vomit have been made, they could not succeed. I have *a case*, which is related in the tenth volume of the 'Journal de Pharmacie,' in which attempts were made to give emetics without success."

Dr. Letheby is asked by the same counsel—"Is the *vomiting* of the pills *before death* inconsistent with what you have known or observed in strychnia-poisoning? *A.* It is not consistent with anything I have observed."

The act of vomiting by Cook here referred to must have been on the Monday night—in the course of the slighter attack from which he completely recovered. On the Tuesday night,

<sup>1</sup> Toxicologie. Tom. iii, p. 47.



at the time the pills containing the strychnia were administered, Jones stated there was a momentary act of vomiting; but this was an hour before any symptoms had commenced—in fact, just as the poison had reached the stomach. There was no vomiting of the pills before death, or while Cook was under the violent symptoms which killed him, and the question and answer were based on a complete mistake.

Assuming, however, that vomiting had really occurred, Sergeant Shee was well aware that Cook had had frequent attacks of sickness during his illness, and that antimony had been found in his body, even in his stomach. He should have therefore asked his witnesses these further questions: “Q. Supposing the animals on which you experimented had been previously well dosed with antimony, would it not be likely that this antimony would give rise to great irritability of the stomach? A. Yes.—Q. Then you would not feel surprised at an effort in the stomach to reject pills, swallowed most unwillingly, whether they contained strychnia or any other substance? A. No.”

The answers of the witnesses as they stand, are not only irrelevant but perfectly inconsistent with the facts observed in strychnia-poisoning. In the case reported by Drs. Lawrie and Cowan, already referred to, notwithstanding the existence of spasms arising from a dose of three grains of strychnia, the patient swallowed three or four doses of an emetic, which was passed to the back of the mouth, and *free vomiting was induced*. This was two hours and a quarter after the ingestion of the poison, and *three quarters of an hour after the first spasm!*

In case No. 2 in the table (p. 78), in which a very large dose of strychnia had been taken, slight vomiting was produced by tartar emetic. In Case No. 11 (p. 82), in which one grain of pure strychnia had been taken in a pill, from which the deceased died in one hour and a half, there was violent sickness; and in a case reported by Dr. Beck,<sup>1</sup> in which half a grain of strychnia caused spasms with opisthotonos, there was occasional vomiting.

Facts of this kind may not be consistent with what the witnesses have themselves observed, but they prove incontestably that the act of vomiting is not inconsistent with poisoning by

<sup>1</sup> ‘American Journal of Med. Science,’ Oct., 1851, p. 535.

strychnia, even supposing that the vomiting had occurred at the time in which it was incorrectly represented to have occurred in the case of Cook.

One other assumed difference is also strongly dwelt upon by the witnesses for the defence, namely, *the length of time which elapsed before symptoms came on* (about an hour or an hour and a quarter), although evidence had been given at the trial that a delay of symptoms had been distinctly observed in a case of poisoning by strychnia for the period of one hour after the poison had been taken. Mr. Nunneley and Dr. Letheby both assign this long interval as one of the reasons why they would not ascribe the death of Cook to strychnia. Their experience, they admitted was chiefly derived from experiments on animals. Mr. Nunneley had experimented on sixty animals, and the time of occurrence of symptoms from the ingestion of the poison was from *two to thirty minutes*, more generally about *five or six*. Dr. Letheby stated that he had seen "some dozens" of cases of the administration of strychnia to animals, and the average time when the symptoms began was a quarter of an hour. He had seen them begin in five minutes, and the longest interval was *three quarters of an hour*. In answer to a question, he said, "I have never witnessed such a long interval between the administration of the poison and the coming on of the symptoms as in this case."

The case No. 3 in the table (p. 79), of which a report was published in the 'Lancet' for August 31st, 1850, p. 259, was given by me in evidence at the trial. It is that of a girl who had taken one grain and a half of strychnia. *No symptoms appeared for rather more than an hour after taking the poison*. In 1848 Dr. Anderson published a case of poisoning by strychnia, in which a man took, by mistake for muriate of morphia, three and a half grains of strychnia. No symptoms appeared for *two hours and a half*.<sup>1</sup> In the case of Drs. Lawrie and Cowan, already referred to, which occurred on the 11th June, 1853, an adult took three grains of pure strychnia, perfectly dissolved in a mixture of spirit, sulphuric acid, and water. Like Mr. Cook, he went to sleep, and about *an hour and a half* afterwards he suddenly awoke in a spasm, uttering loud

<sup>1</sup> 'Edinburgh Monthly Journal,' February, 1848 p. 566.

ries, which alarmed the household.<sup>1</sup> The witnesses for the defence were no doubt correct in saying that in their experiments on animals, they had never met with a longer interval than forty-five minutes; but the above facts in reference to the human subject, one of which was actually in evidence before the Court, shows the extreme danger of a witness attempting to solve a question of this kind by little or no experience on the human subject, and by a limited experience on animals. Even so far as animals are concerned, the statement is extraordinary. In one experiment by Dr. Christison, no symptoms appeared, in a rabbit, after a fatal dose for *an hour and three quarters* (see p. 116.); and in an experiment by Dr. Rees and myself, in which one grain of strychnia was given to a rabbit, there were no symptoms for *an hour and five minutes* (p. 109). Some facts have been communicated to me in which five or six hours have elapsed before symptoms appeared in animals. (See pp. 120, 146.) In fact, it generally depends on the dose, on the rate of absorption, on the mode in which the poison is given, and various other circumstances. It must be, however, perfectly clear that with experience accessible from other sources, there was not the slightest justification for the statement that the interval which had elapsed before the symptoms showed themselves in Cook, was so great as to be any real objection to their having arisen from strychnia. In questions having this general bearing on scientific truth, witnesses are not expected to rest on their own experience; and certainly no case was ever tried in our Courts, in which so great a freedom of reference to all facts connected with poisoning by strychnia, was conceded. Mr. Nunneley, indeed, did not hesitate to refer to a case, in the 'Journal de Pharmacie,' in support of his view, that spasm would prevent vomiting in strychnia poisoning.

So far with the objections from *symptoms*. The witnesses when asked respecting *appearances* in the dead body, stated that the condition of the heart in Cook, was not consistent with death from strychnia. Dr. Harland deposed that "the heart was contracted, and contained no blood." In the private memorandum sent to me, the only notice of the state of this organ was—"the heart was of natural size, and in every part

<sup>1</sup> See the case, 'Glasgow Medical Journal,' Part xiv, July, 1856.

healthy." The blood is stated to have been fluid in all parts of the body.

It was admitted by the witnesses on both sides, that in their experiments on animals, the heart was generally found full of blood, especially on the right side; but the question arises, whether a point of this nature can be determined better by the results of experiments on animals, than by observations on man. On reference to the table of fatal cases of strychnia (p. 78), the variable nature of the appearances in the body, will be at once apparent. Age, sex, the dose, state of health, and peculiarities of system, will account for the differences which appear, and if such differences exist among human beings, *à fortiori*, they may exist between human beings and animals. A sufficient number of facts, however, have been accumulated to show that "animal experience" would lead to very fallacious conclusions. In case No. 2 (p. 78), in the table, fatal in one hour and a half, the *heart was found flabby and empty*,—the *blood every where fluid*. In case No. 4 in the table (p. 79), that of Agnes French, given in evidence at the trial, in which the patient died in about an hour, the *cavities of the heart were quite empty*, the muscular fibre of the heart was stiff. In case No. 10 (p. 82), that of Mrs. Smyth, also given in evidence before the Court, *the heart was contracted and perfectly empty*. In case No. 13 (p. 83), in which the patient died in about an hour after taking one grain and a half of strychnia, the heart was found, by Dr. Lonsdale, to be healthy, *empty and unnaturally atonic*. In No. 14 (p. 83), death from a large dose; the heart is described as flabby, and containing *only an ounce of blood in the left ventricle*. In No. 15 (p. 84), we are told by Dr. Geoghegan, an accurate observer,—a case in which five grains of strychnia had proved fatal in twenty-five minutes—that *the heart was firmly contracted*, and its cavities contained a *very small quantity* of dark fluid blood. Thus, in six out of *ten inspections* of *fifteen* fatal cases of poisoning by strychnia, the heart was either found empty, sometimes contracted and sometimes flabby, or there was not that fulness of the cavities which, from observations on animals, the witnesses had pronounced to be a necessary and constant attendant to poisoning by strychnia!

To these facts other cases might be added; for instance, that of Mrs. Dove, although the emptiness here was referred

by one of the witnesses to the draining of the organ by reason of the head having been first examined. This does not appear, however, to afford a satisfactory explanation. In cases Nos. 10 and 14 (pages 82, 83), in which the heart was entirely or nearly empty, the head was not examined; and in cases, 5, 8, 12, in which the cavities of the heart contained much blood, the head had been examined. In an American case, of which I have a private note, that of Mr. G. W. Greene, tried at the Chicago Circuit Court, December, 1854, for the murder of his wife by poisoning her with strychnia, it was stated in evidence by Dr. Freer, who examined the body, that the heart of the deceased *was healthy, but empty*, as well as the large vessels near it. In the case of Azenath Smith, tried in Canada, in 1851, for the murder of her husband by administering strychnia in pills, Dr. Dupson, who gave evidence on the occasion, said that he found the heart of the deceased, *empty in all its cavities, and healthy*.<sup>1</sup>

In a recent case of poisoning by nux vomica, which is, in fact, poisoning by strychnia, reported by Mr. Davies, the same condition of the heart was observed. The quantity of nux vomica taken was half an ounce, and the patient died in two hours after the commencement of the symptoms. The body was inspected seventy hours after death, and among other appearances, he states that "to the touch the heart felt quite hard; its structure appeared quite healthy; its smallness may be accounted for *by the whole of its cavities being empty*. The *articles and ventricles did not collectively contain a drachm of blood*."<sup>2</sup>

These facts are sufficiently numerous to show that the empty condition of the heart found in Cook, is perfectly consistent with death from strychnia-poisoning, as it has occurred in human beings, however it may appear to militate against the results of observations made on animals. My own experiments, on a few rabbits, are in accordance with those of the witnesses for the defence, who have had the advantage of a larger experience in this direction; one having put to death by strychnia upwards of sixty animals, including dogs, cats, rats, mice, guineapigs, rabbits, frogs, and toads; the other having

<sup>1</sup> 'London Medical Gazette,' September 19, 1851, p. 517. For another case, see Appendix, p. 142.

<sup>2</sup> See 'Medical Times and Gazette,' February 9, 1856, p. 149.

destroyed the indefinite number of "some dozens"—"the right side of the heart was always full of blood." At the same time, there appears to be some difference even among animals. Mr. Startin, who has recently published Dr. Booth's case (No. 1 in the table, p. 78), states that it was not possible in that case to procure permission to examine the chest. From experiments on dogs, instituted at the time by himself, he states that both the *full* and *empty condition* of the heart may be found, and that this was accounted for in the animals by death taking place either during an inspiration or during an expiration.<sup>1</sup>

This question is obviously of great importance in regard to diagnosis in future cases. Is this emptiness of the heart to be or not to be taken as one of the appearances likely to be met with in poisoning by strychnia? Is it a sign of some other fatal disease; and if so, of what? It cannot be referred to idiopathic asphyxia. The cases already collected of strychnia-poisoning, to which may now be added that of John Parsons Cook, show, whatever may be the results of observations on animals,<sup>2</sup> that emptiness of the heart, with sometimes flaccidity and sometimes tonic contraction, is a state which has been frequently observed in death from strychnia, and thus it may be met with in future cases. I am indebted to Mr. Poland for some information respecting the condition of the heart in death from idiopathic or traumatic tetanus.<sup>3</sup>

Of the occasional congestions found in the membranes of the brain and spinal marrow, in the substance of those organs and of the lungs, it is unnecessary to speak. There was nothing in respect to these parts in the case of Cook, which was at all opposed to the supposition of death by strychnia.

<sup>1</sup> 'Medical Times and Gazette,' July 12, 1856, p. 36. See also experiment by Mr. Horsley, p. 120, *post*.

<sup>2</sup> If there are differences in the *symptoms*, there may be differences in the *appearances*, produced by strychnia on man and animals. According to my experiments, spasmodic closure of the jaw sets in among the early convulsive symptoms in animals. This is certainly not the case in the human subject. According to observation, it appears late. Should we be justified in inferring from this effect on animals, that fixed closure of the jaw is one of the first symptoms of strychnia-poisoning in man,—and because in Cook's case, the jaw was not closed, and he was able to swallow "antidotal" pills within ten minutes of death,—that his symptoms were not referable to strychnia?

<sup>3</sup> The reader will find this information in an Appendix to this paper.

The only conclusions which it appears to me we can draw from this inquiry are—

1. That the symptoms and appearances observed in the case of John Parsons Cook are not only consistent with death by strychnia, but such as from experience in other cases in the *human subject* we may commonly expect to find as a result of the action of this poison.

2. That the symptoms and appearances, taken as a whole, are not reconcilable with death from any other cause.

3. That the objections urged to this cause of death by the witnesses for the defence have no real foundation, and are directly opposed to observations hitherto made in cases of death from strychnia.

By a comparative examination of the table at p. 78, it will be seen that the case of Cook (No. 16, p. 84) is in close accordance with the general order and progress of the symptoms, as well as with the appearances met with after death in the fifteen fatal cases which are reported. If a medical witness insists upon selecting isolated symptoms or appearances for comparison, he will of course find an ample field for dispute in comparing known cases of poisoning by strychnia with each other. There are some in the table which differ more from each other than Cook's case differs from them. If he ignores facts, already recorded by others with no other view than that of contributing to the general stock of scientific knowledge, if he persists in confining his opinion to his own limited experience of cases in the human subject, or if, in ignorance of reported cases in human beings, he confines his inferences to results obtained from experiments on animals, he may be so far accurate in his evidence, but he will at the same time seriously mislead a Court of law. A jury placing reliance upon such partial evidence as this, might be led to return a verdict quite contrary to the truth. The fact is, a scientific witness is called not merely to inform the Court on what he has himself seen, done, and known; but, in addition, to give the general results of scientific experience from all authentic records. It is only by this that the truth can be really ascertained.

The solicitors for the prosecution in this case, therefore,

acted most wisely in placing before the jury, in all their details, and irrespective of any medical theories or conclusions derived from experiments on animals, three clear and unambiguous cases of poisoning by strychnia. A suggestion was made on the part of the defence, to the effect—that in the facts in which these cases strongly resembled that of Cook, they were of an exceptional kind (e. g., Mrs. Smyth's case, p. 82); but the information thus laid before the jury enabled them to throw aside much of the medical sophistry with which the evidence had been overlaid, and to take a common-sense view of the facts. A strong complaint has been made on the conflicting opinions given regarding the cause of death; but such conflicting opinions must always be given if medical facts are either not known, only partially known, or wholly ignored by the witnesses. It is impossible, under such circumstances, that there can be agreement or consistency in the evidence. There is no resource but to test the accuracy of an opinion, not by the individual experience of the witnesses, but by the general experience of the medical profession.

The next important question which arose out of the evidence given at this trial was the following:

CAN A PERSON DIE FROM POISON AND NO POISON BE FOUND BY CHEMICAL ANALYSIS IN THE BODY.

I here put this question generally. At a very early period in the progress of this case, it was found that if the defence failed in assigning the symptoms of Cook to some latent disease, the only point on which it could rest was this:—If it be alleged that a person has died of poison, let it be produced in a visible and tangible form; if it cannot be produced, then, supposing proper skill to have been employed, the only inference to be drawn is, that no poison was taken, and that death was caused by disease. This is bringing the question of death from poison to a very simple issue indeed. It is casting aside physiology and pathology, and requiring our law-authorities to place entire and exclusive confidence in the crucible and test-tube of the chemist. But has Organic chemistry, with all its modern advances, yet reached a point that no death can occur from poison, speaking generally, except the poison be still



found either in the stomach, the tissues, the blood, the excretions, or in all of these parts at one and the same time? Is the viper-poison easily revealed by tests? Can the poison of rabies, producing one of the most formidable convulsive affections known, namely hydrophobia, be detected in the tissues? Is there any chemical process by which the poison of the ordeal bean of Africa, or even of the common laburnum, the seeds of the *Ricinus communis*, the poisonous fungi, darnel, and the sausage-poison of Germany, the poison of the *cenanthe crocata*, — can be separated and demonstrated to exist after death in the blood and tissues? If not, then the allegation that no person can die from poison except the poison be found in the body is a mockery, a delusion, and a snare, admirably adapted to cover a multitude of deaths from poison, which, but for this dogma, might be revealed by pathology and physiology. It is all the more dangerous, because the history of crime shows us that the arts of the murderer, especially of the scientific or professional murderer, are daily becoming more refined. I could add largely to the list of poisons which either by their nature, by their tremendous power in very small doses, or by the mode in which they are introduced into the system, might infallibly produce death without leaving a physical or a chemical trace of their presence in the body. I forbear to do this. Such an enumeration would undoubtedly serve my purpose of refuting that which I believe to be a gross and dangerous error on the part of some of the chemists who gave evidence for the defence at the trial of Palmer; but it would be at the cost of making public, means of death and modes of perpetrating crime which it would be dangerous to promulgate. The fallacious doctrine here broached for the temporary purpose of saving the life of a wretched criminal was, however, such as to receive, at least for a time, a large amount of popular support. There was an astonishing plausibility about it, especially in the form in which it was almost daily circulated in newspaper paragraphs emanating from the solicitor for the defence or his agents. It only required bold assertion, and the chemical differences hitherto admitted by chemists to exist between organic and inorganic poisons at once vanished. Alarm was also spread and allowed to pervade the public mind by the allegation, that unless poisons were invariably detected in, and separated from the dead body,

in cases of alleged poisoning, any innocent person might be convicted of murder by poison when the death was really due to some latent disease. It is fortunate that the jury in Palmer's case have, by their verdict, given the death-blow to this novel and dangerous doctrine, and have shown that twelve men may be as safely directed to a just decision by the views of pathologists and physiologists as by the assumptions of chemists. This is as it should be. Chemistry may detect a poison ; but it fails, without the aid of physiology and pathology, to show whether it was or was not the cause of death ; and, in some instances, it cannot enable us to determine whether the poison was introduced into the body during life or after death. Even with regard to the poison in question in this particular case,—*strychnia*,—this substance is now so extensively employed as a medicine, that the discovery of traces of it in the stomach, blood, and tissues (assuming that the processes used are satisfactory) would not justify an allegation of death from poisoning by it. *The symptoms must be made known.* The “tetanic complications” which it ordinarily produces in the body when taken in poisonous doses must be clearly established, and a judgment must be based on these symptoms. We are not, therefore, to suppose, as the public have been erroneously led to imagine, that toxicology and chemistry are convertible terms, that the finding of poison in a body is a proof of death from it, and the not finding the poison is a proof of death from some natural cause.

It is, I think, satisfactory to science that no physician or chemist of any authority as a toxicologist, could be found to support this novel doctrine. I have reason to know, that many men highly distinguished for their chemical and medico-legal knowledge, in the three kingdoms, had been urgently pressed, on the part of the defence, to come forward and maintain that the discovery of poison in the body was a *sine quâ non* in every case of actual poisoning ! It is highly creditable to them that they positively refused, on this ground, to appear in the case. Only two gentlemen at all known to the public could be found to support this view ; namely, Mr. Herapath, of Bristol and Dr. Letheby, of the London Hospital, both of whom were, by a singular coincidence, engaged for the defence in the case of another great criminal, Tawell, who

was convicted and executed for poisoning Sarah Hart, by prussic acid, at the Aylesbury Lent Assizes, in 1845.<sup>1</sup>

Dr. Christison, a writer of deservedly high authority, and a witness for the Crown in Palmer's case, says, in reference to the question which we are here considering, that the doctrine that "no charge of poisoning can be established without the discovery of poison in the body or in the evacuations, is a great error. Under this doctrine few criminals would be brought to justice were they to resort to a variety of vegetable poisons, which, in certain seasons, are within the reach of every one."<sup>2</sup> Dr. Geoghegan, Professor of Medical Jurisprudence in the Royal College of Surgeons, Ireland, remarks, that "the information derivable from a chemical examination of the contents of the stomach, although, of course, a most important element in medico-legal investigations connected with poisoning, does not carry with it that exclusive weight which is attached to it by those of the profession who are uninformed in forensic medicine; nay, further, although a positive decision may be occasionally arrived at from other single sources of evidence, the *chemical* examination alone cannot decide the cause of death."<sup>3</sup>

<sup>1</sup> In this case, my friend the late Mr. Cooper, who conducted the chemical analysis, informed me, that he procured a *grain* of anhydrous prussic acid by distillation from the stomach of the deceased, and he gave evidence to this effect at the trial. This was quite sufficient to account for death. The presence of the poison could not be disputed; but it was here contended, of course by the sanction of the chemists, that this quantity of prussic acid had been spontaneously generated by a chemical reaction in some apple-pips, which the deceased was supposed to have swallowed with the pulp of apple, found in the stomach. The defence was essentially of a pseudo-scientific and chemical nature. It utterly failed, because it was opposed to common sense and common experience. A medical witness may, however, safely rely upon the fact, that when there is a sufficient stimulus for the display of chemical ingenuity in "shaking" the evidence of witnesses engaged in a public prosecution, it will always be forthcoming. If the poison is not found in the body, then it will be urged that there is a total want of proof of poisoning; if it be found, the witness will have to meet the objections, that there was not enough in the body to account for death, or failing this, that the tests and chemical processes employed, were not satisfactory; in fact, not such as "higher adepts" in science would resort to, or lastly, if this position should be untenable, then that the poison was the product of some spontaneous chemical changes within the body, and that it was not administered to the deceased.

<sup>2</sup> 'Treatise on Poisons,' 4th edition, p. 71.

<sup>3</sup> Case of Poisoning by Monkshood, 'Dublin Medical Journal.'

In accordance with these views, the great principle of jurisprudence in a criminal charge, now confirmed by the verdict in Palmer's case, is not that a poison should be invariably found in the dead body, but that there should be satisfactory evidence of death from poison; and this may be had when chemical tests, even in the hands of "adepts," fail to reveal its presence. To affirm that innocent persons may be placed in danger from the adoption of such a principle, is absurd; it is equal to affirming, that on a question of poisoning, chemistry alone is to be relied on; and that persons accused of this crime are to be convicted or acquitted according to whether a chemist believes, by the production of certain colours, that he has extracted the fifty-thousandth of a grain of some alkaloid from the thirty-second part of a liver, or that he is prepared to swear to the presence of the substance from the form of a crystal seen under a polarizing microscope, and calculated to weigh the ten-thousandth part of a grain! The danger, in my opinion, is quite the other way. By placing a blind reliance upon such extreme results, the accuracy of which cannot be determined or their value judged by Courts or juries, there is very great risk that a really innocent person may be condemned on erroneous evidence. At any rate, his only chance of safety would be in the corrective proofs derivable from physiology and pathology; that is to say, in the nature of the symptoms preceding death, and in the appearances presented by the dead body. On the other hand, as this extreme nicety of detection is not admitted to be a property of organic poisons by men of experience and repute as toxicologists, who, in their scepticism, can have no object but the public good, it follows that many criminals would escape, if such a doctrine were once admitted by our Courts of law.

It has been supposed that Palmer's case presented a novelty in this respect, and it was rather industriously asserted in certain publications emanating from the defence, that no case could be pointed out in which there had been a conviction for murder by poison and the poison not found in the body. The case of Dr. Castaign, who was tried and executed in Paris, in 1823, for the murder, by poison, of his friend and companion, Augustus Ballet, presents many points of resemblance to the case of Palmer, even in the kind of defence set up. Castaign

had been a pupil under Orfila, and was charged with having made use of his knowledge of poisons to take away life. The prisoner was intimate with two brothers, who were young men of fortune. One of them, Hippolyte Ballet, who had been for some time an invalid, and was attended in his illness by his friend Castaign, died rather suddenly, in October, 1822. No suspicion then arose that he had died from other than natural causes. He had made his will in favour of Castaign, bequeathing nothing to his brother. Castaign afterwards surrendered the will to the surviving brother, Augustus Ballet, for a large sum of money. About seven months after this, the prisoner, who had been travelling with Augustus, unaccompanied by servants, put up at an inn near Paris. In the course of the evening Augustus complained of illness, and Castaign prescribed for him sugared wine. Castaign left his companion at four o'clock in the morning, to take, as he said, a walk; but it was proved that he went to Paris, and procured, at a druggist's shop, twelve grains of tartar emetic and half a drachm of acetate of morphia, assigning as a reason for the purchase at this unseasonable time, that he required it for the performance of experiments on dogs and cats.<sup>1</sup> He immediately returned to Ballet, who was still lying ill, and he prescribed for him cold milk, which he himself administered to him. In five minutes Ballet was seized with convulsions, and, in half an hour, with vomiting and purging. A physician was called in, who, on the representation of Castaign, treated the case as cholera

<sup>1</sup> In Palmer's case, there was some attempt at a suggestion of this kind, but it was too improbable to be carried out. Some dogs were said to have worried the prisoner's brood-mares, and for the purpose of poisoning them, it was to be assumed that the prisoner had secretly procured by gift, three grains of strychnia, late on the Monday evening, within three hours of Cook's first attack, and six grains at another place on Tuesday, within twelve hours of the second attack and death! The strychnia so procured, had evidently been disposed of in some way; for of the three poisons purchased on the Tuesday, prussic acid, Battley's sedative solution, and strychnia, the first two were found on the prisoner's premises unopened, but the strychnia had disappeared. It would have been difficult to persuade a jury, that for the purpose of poisoning dogs three grains of strychnia were procured secretly at a late hour on Monday night in November, or that the whole quantity (nine grains) was such as a man seriously intending to destroy dogs would have procured in two separate quantities, under the circumstances. It was clearly impossible to account in a way consistent with innocence for the possession of the poison.

morbus. In a few hours the patient became quite insensible; he was unable to swallow, was bathed in a cold sweat, with a small pulse, stertorous breathing, a contracted pupil, a hot skin, the jaws locked, the neck rigid, the abdomen tense, and the limbs affected by spasmodic convulsions. In this state he died, about thirty hours after the first symptoms. The only morbid appearances on inspection, were congestion of the brain and serous effusion in the cerebral membranes. The stomach and its contents, as well as other parts of the body, were submitted to a very minute and elaborate chemical analysis, by Vauquelin, Lherminier, Magendie, Barruel, Segalas, and Pelletan, comprising the most eminent organic chemists in France. Chaussier and Pelletan attended the investigation on the part of the prisoner. Morphia, strychnia, and brucia, were especially sought for, by processes and tests quite appropriate for use in the present day.<sup>1</sup> The conclusion come to by these chemists was, that there was no trace of any one of these poisons in the body.

The medical defence was, that as no poison was found in the body it was not a case of poisoning. Orfila, in giving his evidence, said that poisons might cause death, and yet not be detected, owing to their removal by vomiting or absorption. Chaussier, who appeared for the defence, was asked whether the acetate of morphia could be detected in the dead body? *Yes, to a molecule.* But when it is absorbed, is it then possible to find it? It requires a long time to be absorbed, and when *the poison cannot be found the corpus delicti is wanting.* Does acetate of morphia produce a dilatation of the pupil? *Yes.* You do not agree with Dr. Orfila? I have experience that Dr. Orfila does not possess. In spite of the non-detection of poison, however, the jury were quite satisfied that Augustus Ballet had died from acetate of morphia, administered by the prisoner, and he was convicted and executed.

This is not a solitary case. A woman of the name of M'Conkey was tried at the Monaghan Lent Assizes, in 1841, for the murder of her husband by poison. The analysis was intrusted to my friend, Dr. Geoghegan, Professor of Medical Jurisprudence in the Royal College of Surgeons, a good chemist and

<sup>1</sup> The test used for morphia is that which is now employed by toxicologists,—a bitter taste, and the colour produced by nitric acid.

an experienced medical jurist. The stomach, in this case, had not been cut open and its contents mixed with the intestines in one large jar, but they had been carefully and separately packed and sealed. In stating the result, he says: "After a careful chemical examination of the coats of the stomach, its mucus, and the contents of the intestine, the particulars of which it is unnecessary to detail, I was unable to detect any trace of poison." The symptoms, however, were clearly those of poisoning by monkshood or aconite, and the morbid appearances were corroborative. The woman was very properly convicted and executed. Previous to execution, she confessed that her husband had been poisoned by the root of "*blue rocket*" (*aconitum napellus*), which she said had been mixed with pepper by an acquaintance of her's, and sprinkled over some greens, which her husband had eaten at his dinner. She denied, however, any knowledge of the transaction at the time of its occurrence, but this denial was quite inconsistent with moral circumstances. Dr. Geoghegan refers to other instances of general poisoning, in which the fact of poisoning, *in the abstract*, was determined from medical and moral circumstances, without any exact evidence as to the nature of the *special poison* employed; *e.g.* those of Thom, for the murder of the Mitchells, at Aberdeen, 1821; of Mary Ann Alcorn, Edinburgh, 1827; and of Rachel Shannon, tried at the Cork Lent Assizes, 1837.<sup>1</sup> "There can be very little doubt," he observes, "that many additional cases of this kind would have come to light, had medical men devoted more attention to the discrimination of the circumstances under which an opinion in favour of poisoning in the *abstract* may be given, although there be *no chemical indication of the special agent*. It should be borne in mind, that in those instances in which the medical inspector, although unable to give a *decided* opinion, has good grounds for concluding the fact of poisoning to be highly *probable*, it becomes his duty to state this distinctly, when examined, as possibly such an opinion, when conjoined with strong moral evidence, may enable the jury to render the cause of death (constructively) certain."<sup>2</sup>

In the course of some years devoted to toxicological

<sup>1</sup> Jebb's 'Reserved Cases,' Dublin, 1841, p. 209.

<sup>2</sup> See his paper on Poisoning by Monkshood, in the 'Dublin Medical Journal.'

researches, I have met with several instances of poisoning by vegetable, and some by mineral substances, in which no trace of poison could be detected in the body, although the stomach and its contents had been properly preserved. It was not until the occurrence of Palmer's trial, that I had ever heard facts of this kind seriously disputed. It has been hitherto considered that volatile poisons, such as Prussic acid, when taken only in small but fatal doses, are apt to disappear by their volatility, and may no longer be found in the body. It has been supposed that vomiting and purging might get rid of poison,—the person die, and no trace be found in the body. Instances of this I have met with in suicidal deaths from sulphuric and oxalic acids, and in several cases of undoubted poisoning by opium in a liquid state. It has been usual to consider that rapid absorption and elimination might account for the disappearance of some poisons, and that, for this reason, if the dose were small and just sufficient to prove fatal, the chance of finding it would be less than if it were large. Dr. Christison, writing of opium, in the last edition of his work on Poisons, says, "but according to my own observations, the poison *will often disappear in a short time*, so far as to render an analysis abortive. Thus, in the case of a young woman, who died five hours after taking not less than two ounces of laudanum (which would be equivalent to at least fifty grains of opium), I could apply, to the fluid procured from the contents of the stomach, only the test of its taste, which had the bitterness of morphia. In the case of another young woman, whose stomach was emptied by the stomach-pump, four hours after she had taken two ounces of laudanum, I could obtain from the evacuated fluid, when properly prepared, only the indications of the presence of morphia, supplied by its bitterness with the imperfect action of nitric acid, and the indication of the presence of meconic acid supplied by the imperfect action of perchloride of iron. In a third case, where the stomach was evacuated two hours after seven drachms of laudanum (equivalent to at least twenty-one grains of opium), had been swallowed, even the first portions of fluid withdrawn, had not any opiate odour, and did not yield any indication of the presence even of meconic acid. Now, on the one hand, the quantity taken in these instances is rarely exceeded in cases



of poisoning with laudanum; and, on the other hand, the interval during which it remained in the stomach subject to vital operations, is considerably less than the average in medico-legal, and above all in fatal cases. It may be laid down, therefore, as a general rule, that in poisoning with opium, the medical jurist, by the best methods of analysis yet known, will often fail in procuring satisfactory evidence, and sometimes fail to obtain any evidence at all of the existence of poison in the contents of the stomach."<sup>1</sup> Such is the testimony of an accurate observer regarding the means of "infallibly" detecting in the body a poison which causes annually, in this country, as many deaths as are occasioned by all other poisons taken together. This testimony too is not got up to meet a particular set of circumstances, but it is the result of long and accurate observation.

When the quantity of opium taken is small, and the person survives a few hours, neither the morphia, nor the meconic acid, for which a chemist looks, can be detected. In one case which I had to examine, a child about six years of age, died from three-quarters of a grain of opium given in divided doses. It survived thirty-six hours from the time of commencing the medicine, and had not taken any portion for some hours before it died. On examining the stomach and bowels, there was not the smallest trace of opium, of morphia, or meconic acid to be detected. At the maximum, the quantity of morphia which killed this child, would not have exceeded one tenth of a grain, distributed in doses over thirty-six hours. Infants have been frequently killed in a few hours by doses of opiate preparations corresponding to the fifth, the eighth, the tenth, and even the twelfth part of a grain of opium, equivalent, in the first case, to one fortieth of a grain, and in the last case, to one ninety-sixth of a grain of morphia. Dr. E. Smith, has lately recorded a case in which an infant, seven days old, died comatose eighteen hours after having taken one minim of tincture of opium. Symptoms of narcotic poisoning set in, in about half an hour, and the child never rallied from the effects of the drug. Here not more than one twelfth of a grain of opium, corresponding to the 120th grain

<sup>1</sup> 'Treatise on Poisons,' 4th edition, p. 697.

of morphia, destroyed life ('Medical Times and Gazette,' April 15th, 1854, p. 386). Is it to be supposed, in reference to such cases, that even if there were no absorption and diffusion of the morphia through the body of the child, a chemist could separate this minute quantity of morphia from the stomach? If a man ventured to assert this, no one who knows practically the properties of opium would believe the assertion. The allegation that morphia must in all cases be found, before death is attributed to narcotic poison, would lead to wholesale infanticide, without any chance of proving the perpetration of the crime.

The conclusion, from this inquiry, therefore, is that there are many poisons which may cause death, under circumstances in which they cannot be detected by chemical analysis in the body.

It now remains to be determined, whether the poison strychnia is an exception to the remarks here made, regarding the detection of organic poisons in general.

CAN A PERSON DIE FROM STRYCHNIA, AND NO TRACE OF THAT POISON BE FOUND BY CHEMICAL ANALYSIS IN THE BODY?

The reader will perceive, that the practical answer to this question must consist, not in the application of chemical tests to a substance which we know to be strychnia out of the body, but to our means of separating it from the solids and fluids of the body in a case in which we do not know, but in which we are called upon to prove that it has been taken. This inquiry, in fact, involves:

1. The chemical processes by which the supposed strychnia may be separated from the solids and liquids of the dead body.
2. The chemical tests applied to determine that the substance so separated, is really strychnia and nothing else.
3. The amount of confidence to be placed in the so-called tests, in a case of alleged murder, where the acquittal or conviction of the prisoner will probably turn upon the answer.

I have put the third proposition in the form above given,

because the occurrence of a case involving this question may be easily conceived, and the trustworthiness of the tests can then, and then only, be duly estimated. I must beg the reader to remember that this is not a question of the adulteration of bitter beer, or any ordinary article of food, or any artificial admixture of poison, in which we are seeking for that which we know to be present; it is one, I must assume, involving the life of an individual, and therefore deserving of serious consideration.

Although contrary to usual practice, but in accordance with the wish of the Attorney-General, who was desirous of giving every possible advantage to those who were defending William Palmer, Dr. Rees and I furnished to the prisoner's solicitor the subjoined statement regarding the processes pursued by us for the detection of strychnia and antimony in the body of Cook.<sup>1</sup>

<sup>1</sup> I may here observe, that copies of the medical evidence, including the reasons for the opinions of the witnesses, were placed in the hands of the prisoner's attorney some time before the trial. His counsel, legal and chemical, therefore, had full opportunity of anticipating the statements of the witnesses for the prosecution, and framing their questions for cross-examination accordingly. There can, I conceive, be no objection to this course, when scientific questions are likely to be dealt with *bonâ fide* and with a view to public justice, by witnesses consulted for the defence; but even then, the exchange of documents should be reciprocal. I need hardly observe, however, to those who are acquainted with the special pleading which may be brought to bear on scientific questions, to the confusion of the Court and jury, that such a practice must be in the end detrimental to the course of justice.

The verdict in Palmer's case certainly shows that no evil attended it on this occasion, but from its exceptional nature, this case can hardly be quoted as favorable to the practice. If carried out to an equal extent in future cases, it must, in my opinion, be destructive of all confidence between those who are consulted and those who consult. There may, it is well known, be a good use and a bad use of such documents thus confidentially furnished. There are cases in which the object of the defence is not any abstract idea of public justice, but *per fas aut nefas*, the "saving of the life" of the person charged with murder. A medical witness who knows that his chemical processes and opinions are to be submitted to the judgment of hostile critics, employed especially for this purpose, may be induced either to decline the responsibility of the investigation altogether, or to place his statements in such a form as to render the prosecution nugatory. If the rule be good for criminal, it will be equally good for civil cases; if good for medicine, it is equally good for law; and there would be no reason why counsel for prosecution and defence should not furnish each other before trial with abstracts of their intended speeches and the points of evidence on which they intend especially to rely, as well as lists of the scientific witnesses whom they intend to call, including those kept back because

## PROCESS FOR DETECTING STRYCHNIA.

1. *Analysis before the Inquest.*—Stomach well washed with cold distilled water, to remove every soluble substance from surface; pyloric half of the stomach reserved, and one half of the washings. The cardiac half, with one half of the washings mixed with alcohol about '840, acidulated with diluted sulphuric acid, and warmed to about 170°, frequently stirred and allowed to digest for twenty-four hours. Liquid filtered off through wet filter; pale straw-colour; acid reaction; not bitter to taste. About one fourth of this liquid (amounting to two fluid ounces) was neutralized by carbonate of potash, and gently evaporated to dryness. The dry extract was treated with warm rectified spirit until exhausted. This was filtered. It formed a pale-coloured liquid. Again tasted by Dr. Rees and myself; no perceptible bitterness. A portion of this liquid was evaporated gently on a wide dial glass on sand, and films of a fine crystalline substance were left on the glass.

Sulphuric acid and powdered bichromate of potash produced, with these films, a brownish discoloration, and subsequently, green oxide of chrome. A portion of the residue, evaporated in another glass, gave a purple-reddish tint with sulphuric acid and bichromate of potash, the colour becoming of a deeper red by exposure.

The effect was very similar to that which we have obtained from small traces of bile with saccharine matter and sulphuric acid. Our conclusion was that there was no chemical evidence of the presence of strychnia. A portion of the drainings of the jar were tested for strychnia by a similar process, but the alcoholic acid extract had no bitter taste, and bichromate of potash and sulphuric acid gave, with the evaporated residue, only green oxide of chrome.

2. *After Inquest.*—Another portion of the alcoholic acid liquid (which had been reserved) was subsequently tested by Graham's process, *i. e.*, by animal charcoal, but the result was not satisfactory.

Dr. Rees made a subsequent trial on the pyloric half of the stomach (by diluted hydrochloric acid warmed, and followed by animal charcoal and subsequent digestion in alcohol). In this, as well as in the preceding trial, bichromate of potash, ferricyanide of potassium, and sulphuric acid were used, but the results were equally unsatisfactory. There was a change of colour on the addition of the tests, but no clear or positive evidence of the presence of strychnia.

## AS TO DETECTION OF ANTIMONY.

The process used was that of Reinsch, *i. e.*, by a mixture of hydrochloric acid and water, producing a deposit on copper foil and copper gauge.

The different organs mentioned in the report yielded only slight traces. An acid

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their opinions were adverse. Even scientific gentlemen who allow themselves to be employed for such purposes as those above indicated, would object to this mode of dealing with their own facts and opinions. In short, it is difficult to understand how the criminal and civil business of the country could be conducted on such a principle. I make these remarks rather in reference to future cases, that medical men who venture to give opinions in questions of alleged poisoning may know in what position they may really stand.

decoction (hydrochloric) of the pyloric half of the stomach yielded, with zinc, a black deposit, allowed to collect for some days, when this was digested in nitro-hydrochloric acid, and evaporated to dryness. On removing the chloride of zinc by water, a white insoluble sediment was obtained, which, when treated by hydrosulphuret of ammonia, acquired a reddish-brown colour. The deposits on copper presented, when recent, the peculiar violet colour of antimony, and gave, when heated, no sublimate of octohedral crystals. They possessed all the characters of antimonial deposits. A portion of the antimony was converted to antimoniate of soda by deflagration with nitrate of soda, and this compound was subsequently tested, and its antimonial nature established by the action of a current of sulphuretted hydrogen on the solution, acidulated with hydrochloric acid.

In our judgment, the chemical evidence of the presence of strychnia, *on a trial for murder*, should be as clear, distinct, conclusive, and satisfactory, as in the case of arsenic or any other detectable poison; and although we obtained, in this instance, by the use of the tests, certain changes of colour, which an ardent imagination might, I believe, have easily construed into proofs of the presence of strychnia, we declined to take this view, and concluded, from the results above mentioned, that in the parts examined by us, namely, the coats of the stomach and the drainings of the jar, there *was no evidence of the presence of strychnia*. Mr. Sergeant Shee, in his address to the jury, gives to Dr. Rees and myself, credit for having done "all that the science of chemical analysis could enable men to do to detect the poison of strychnia." This observation was of course only intended to influence the jury in relying upon his argument, that "strychnia not having been found in Cook's body, under the circumstances of the case, it never could have been there."

I shall only remark, that although there may be a difference of opinion as to the relative efficacy of the processes employed for detecting strychnia, we had satisfied ourselves by a preliminary trial that strychnia, when in sufficient quantity to justify a medical opinion, might be separated and detected by the processes which we had here adopted.

With the exception of the stomach and the drainings of the jar, no analysis for strychnia was made. We knew no process which could be safely relied on for the separation of strychnia from the tissues; and on this ground we directed our analyses of these parts to the detection of antimony and mercury. Some chemists, who have manifested great wisdom

after the event, in reference to this case, would have devoted their entire researches to the detection of strychnia; and they imagine that they could have found it, if not in the stomach and liquids of the jar, at least in the liver, spleen, and kidneys—nay even in the *bones* of Cook! They forget, however, that at this time there was no suspicion of death from strychnia, no knowledge of symptoms to indicate it; and assuming that there was a satisfactory method of obtaining the poison in a dry and tangible form from the liver, there was no more reason for searching the tissues for strychnia than for searching them for morphia, brucia, veratria, picrotoxia, or any other of the numerous alkaloidal poisons.<sup>1</sup> Unless gifted with the power of pre-vision, it is not probable that their skill would have been directed to this special research; and without it they must have subdivided the organs into various portions to search for the numerous poisons which can occasion sudden death. If they had not taken this course they would probably have incurred a charge of carelessness, in neglecting to seek for the very poison which might be subsequently proved to have caused death. Dr. Rees and I had no confidence in the processes

<sup>1</sup> I stated, in answer to questions put by the learned sergeant, that we had no suspicion of strychnia, until the 4th December, *i. e.*, after the analysis was completed, and that information of the purchase of strychnia was then for the first time made known to us, by a note from Mr. Gardner. Further, this knowledge did not in any way affect our opinion, that the cause of the death of Cook was still unexplained. It was only after reading Jones's statement of the mode in which the deceased died, and hearing his evidence, and that of Mills, that I came to the conclusion that strychnia must have been the poison used. Ten days had then elapsed, and yet the learned sergeant was allowed, without comment or contradiction, to put the following statement to the jury. "They (Drs. Taylor and Rees) had *distinct information* from the executor and a near relative of the deceased, either personally or through his solicitor, that *he from some cause or other had reason to suspect the poison of strychnia*; they undertook the examination of the stomach, which I think upon the whole evidence, without adverting to that part of it now in detail, you will be satisfied was not in an unfavorable condition for a sufficiently accurate analysis, *with the expectation, that if strychnia had been taken, it would be found, and without any doubt as to the efficiency of their tests to detect it.*" This statement was, upon the counsel's own questions, quite contrary to the evidence given on oath. Juries are desired to dismiss from their minds all that they have heard before entering the Court, the fear being that they may act upon the belief of something which is unproved and untrue. Of what utility is such a rule as this for guiding them to a just verdict, if misrepresentations of scientific evidence are thus allowed to be placed before them.

relied on for the detection of strychnia, morphia, &c., in the tissues; and even had we obtained those results which were considered by the chemists for the defence to be conclusive of the presence of strychnia, we should have declined to base our opinion of the cause of death upon such precarious characters. But in fact, up to that time strychnia had not been discovered in the tissues, to our knowledge, in any fatal case of poisoning by it which had occurred in this country. It had not even been sought for by chemists who had had the opportunity, such as Mr. Herapath. His researches for the poison never appear to have gone beyond the contents of one unmutated stomach! We could not find that Orfila, Christison, or any toxicologist of repute, had detected this poison in, and separated it from the tissues in any one instance, either of a human being or of an animal. For these reasons the tissues were not examined, and it was therefore not in our power to say whether strychnia was or was not present in them. We considered that the tests and processes open to us for such a purpose, were too fallacious to allow us to decide the question by resorting to their use. Another course was, however, open to chemists having greater confidence in their chemical processes, who, to use the language of Sergeant Shee in his address to the jury, "were ready to depose on their oaths, that not only if half a grain, or the fiftieth part of a grain, but I believe they will go on to say that if five, ten, or twenty times less (*i. e.* one thousandth of a grain), *had entered into the human frame at all*, it could be and *must be detected by tests which are unerring.*" I need hardly remark, to those who are acquainted with the properties of strychnia, that this is a rash and unfounded statement, calculated to deceive the jury to whom it was addressed, and to lead them to come to a wrong conclusion on the scientific facts. It was entirely unsupported by any evidence which was adduced in the case.

I have elsewhere stated, that the body of Cook was exhumed on the 25th of January, and the solicitor for the prisoner instructed two medical gentlemen to attend on the part of the defence at the exhumation. The fact that strychnia had not been found in the stomach of Cook was then well known to him; and it was in his power to cause portions of the body to be removed for analysis by his chemists, if he was dissatisfied with the results

which had been obtained by Dr. Rees and myself. We had declined to undertake or to rely upon an analysis of the tissues for organic poisons, but there was no impediment to the chemical witnesses, the "higher adepts" in science employed for the defence, undertaking this investigation. The state of the body on the 25th of January, could not have interfered with their analysis. One of them, Mr. Rodgers, professes to discover with great facility strychnia in the *bones*; and Mr. Herapath deposed on oath, that if this poison had caused death, he could detect it up to the time that the body had become "completely decomposed;" in fact, when it was converted to a "*dry powder*." Nothing could be more positive than this evidence: hence the non-analysis of the residue of Cook's body, when an opportunity was presented, shows that those who were most earnest in defending Palmer, believed that Cook had been poisoned by strychnia, and that it would have been dangerous to intrust the analysis to their own witnesses; to gentlemen, in fact, who professed to detect in the dry pulverulent remains, even the 1000th part of a grain, had it been taken during life! Let us consider the result of such a proceeding, had it been adopted. If the strychnia had been found, and its presence in the body of Cook proved to the satisfaction of the witnesses for the defence, this would have only corroborated the evidence for the prosecution, and sealed the fate of the prisoner. If, on the other hand, they reported that they had not found the poison in the tissues, or that the indications of its presence were not to their minds satisfactory, this would have still left the case for the prosecution to be answered just as it then stood; namely, that the symptoms of Cook could only be ascribed to the effects of strychnia, and the negative results of the analysis would have no more assisted the defence than the negative results already obtained by Dr. Rees and myself.

I may here remark, that various incorrect statements respecting the detection of strychnia in cases of poisoning, have been erroneously attributed to me. It has been represented, that at the inquest on Cook, I had given an opinion to the effect that, after an hour had elapsed, when strychnia had been taken as a poison, it could not be proved to exist in the dead body by chemical tests! My statement was to the effect, that strychnia was not so susceptible of detection after death



as arsenic, and other metallic poisons; that it might destroy life in small doses (*half a grain*);<sup>1</sup> and that, according to circumstances, enough *might be absorbed in an hour*, to kill a person. Further, when thus removed from the stomach by absorption, I knew of no instance in which it had ever been detected in the dead body. This matter was very properly placed before the jury, by the learned Attorney-General, in his opening address:

“Sometimes strychnia is found, at other times it is not. It depends upon circumstances. A most minute dose will destroy life, from half to three quarters of a grain will lay the strongest man prostrate. But in order to produce that fatal effect, it must be absorbed into the system, and the absorption takes place in a shorter or longer period, according to the manner in which the poison is presented to the surfaces with which it comes in contact. If it is in a fluid form, it is rapidly taken up, and soon produces its effects; if not, it requires to be absorbed, and the effects are a longer time in showing themselves. But in either case, there is a difficulty in discovering its presence. If it acts only on the nervous system through the circulation, an almost infinitesimal quantity will be present. And as it is a vegetable poison, the tests which can be employed are infinitely more delicate and difficult than those which are applied to other poisons. It is unlike a mineral poison, which can soon be detected and reproduced. If the dose of strychnia has been large, death may ensue before the whole has been absorbed, and a portion is then left in the stomach or intestines. But if a *minimum* dose has been administered, a different consequence follows, and the whole may be absorbed. Practical experience bears out the theory that I am enunciating. Experiments have been tried, which show, that where the same amount of poison has been administered to animals of the same species, death will ensue in about the same number of minutes, accompanied by precisely the same kind of symptoms; while, in the analysis afterwards made, the presence of poison will be detected in one case and not in another. It has been repeated over and over again, that the scientific men employed in this case, had come to the conclusion that *the presence of strychnia could not be detected by any tests known to science*. They have been grievously misunderstood. *They never made any such assertion*. What they have asserted is this—the detection of its presence, where its administration is a matter of certainty, is a matter of the greatest uncertainty. It would, indeed, be a fatal thing to sanction the notion that strychnia administered for the purpose of taking away life, cannot afterwards be detected! Lamentable enough is the uncertainty of detection! Happily, Providence, which has placed this fatal agent at the disposition of man, has marked its effects *with characteristic symptoms*, distinguishable from those of all other agents by the eye of science.”

These are the words of truth, and an accurate repre-

<sup>1</sup> See in the Table, p. 81, Case No. 9, that of Dr. Warner, who died in twenty minutes from a dose corresponding to less than half a grain of pure strychnia. In the Appendix p. 138, will be found a case in which a child was killed in four hours by the *sixteenth part of a grain!*

sentation of the present state of knowledge in reference to this subject.<sup>1</sup>

The principal reasons for the non-detection of strychnia in the body of a person who has died from its effects may depend on—

1. *The quantity taken.* If the dose be small, from one half to three quarters of a grain, it may be rapidly absorbed and removed from the stomach. It is only the *surplus* of a fatal dose which is found in the stomach after death. If a man swallows a dose of ten, fifteen, or twenty grains, and dies quickly, without vomiting, then the residue, or some portion of it, may be found.

<sup>1</sup> In a letter addressed to Sir George Grey, dated March 17th, 1856, Mr. John Smith, solicitor for the prisoner, thus expresses himself:—"Allow me to call your attention to the Leeds case of poisoning by strychnia, where that poison was found, whereas Dr. Taylor *swore it was not traceable.*" I need scarcely observe, that this is an untruthful representation of what I did state at the inquest on Cook, at which Mr. Smith was not present, and therefore could not of his own knowledge know what I did say. I stated that, *in Cook's case, the poison was not detected*, and that a person might in some cases die from this poison without any of it being found in the body; but I did not say that it never could be traced in any case, or under any circumstances. The comparison of the Leeds with Cook's case was manifestly unfair. Mrs. Dove had been dosed with strychnia, at intervals, for a *week* before her death. The last and fatal dose, obviously a large one, was given *diffused in liquid*, only *twenty minutes* before she died. The medical gentlemen who made the analysis (one of them at least) had a full knowledge of the symptoms from personal observation, and *suspected strychnia before her death.* Their analysis was therefore directed exclusively to this poison. They themselves removed the stomach and secured the contents, and after applying to them various tests and processes during several days, had, it appears, still enough of the poison left in the extract of the stomach to kill four animals! Is there any one feature in which the analyses made in the cases of Cook and Dove, can be placed in comparison, or any fact to justify the untruthful statement contained in the letter to Sir George Grey? A medical witness, however, must always be prepared for an ample amount of misrepresentation when the sole object of a solicitor is "to save the life" of his client. The lives of those who have perished by poison are altogether put out of consideration. This appears to be a grievous wrong without any remedy. A solicitor engaged in a case of this kind has an unbounded license extended to him. If the witness is silent under such false imputations, the class of men who are likely to form the jury will think that he is wrong, and has committed a grave error; if, on the other hand, he publishes a contradiction to such untruths, he is immediately charged with prejudice against the "unhappy" prisoner, who is represented as never having written anything against him! To the circulation of such false statements may, in some measure, be traced the crime of Dove in poisoning his wife at Leeds.

2. *On the time which has elapsed after taking the strychnia, until the symptoms commence.* The longer this interval, the greater the quantity of poison removed from the stomach by absorption. The poison has been found to be diffused through the circulation in nine minutes. If the person dies in ten or twenty minutes from the time of swallowing the strychnia, some may be found. If he lives an hour or longer, the greater portion may be removed by absorption.

3. *On the careful preservation of the stomach and its contents.* If the fluid or solid remaining in the stomach at the time of death is not carefully preserved, there is a great probability, if the residuary quantity be small, that it will not be found.

In testing the case of Cook by these propositions, we may remark—

1. The doses were probably small: on Monday night the quantity was not sufficient to prove fatal. On Monday and Tuesday nights a long period elapsed on each occasion before the symptoms commenced. This is not in accordance with the taking of a very large dose. As a medical man, the prisoner knew well the dose which might suffice to kill; and, after the experience acquired by him of the effects of strychnia on Cook on Monday night, he could so adjust the dose as to destroy him without incurring the risk of leaving any great amount of residue. A dose of from three quarters of a grain to a grain would have sufficed to produce the effects on Tuesday night.

2. An hour and a quarter had elapsed before any symptoms manifested themselves in Cook. If absorption commences in a few minutes, and continues throughout, there was time for the removal of the greater part by this process during the hour and a half that he survived.

3. The stomach of deceased was delivered to us in a condition most unfit for analysis, or for the detection of such a poison as strychnia. I am informed that it was accidentally cut during the examination of the body, and some of the contents had escaped. If put into the jar in the mode described by the witnesses in evidence, it is perfectly clear that there must

have been some tampering with it subsequently.<sup>1</sup> When it reached us, it was laid open from end to end, and *turned inside out*. No person making an inspection for the purposes of a chemical analysis, with the slightest knowledge of his duty, could be guilty of an act of this kind. If he had unfortunately lost the contents, wholly or in part, during the examination, as a rational person he would have preserved as much of the stomach as it was in his power to do; or would have placed it in the jar with the ends secured, and in a different condition from that in which we found it.

The fact that the poison was given to Cook in the form of pill was unfavorable to the adhesion of strychnia to the stomach; since, until the structure of the pill had been broken up, the crystals or powder could not come in contact with the mucous membrane; but it is not necessary that the structure of the pill should be entirely broken up, in order that the poison should operate fatally. A partial solution from the outside would suffice. Since this trial, the results of two experiments, performed by persons independently of each other, one by Mr. Devonshire, a witness in this case, and the other by Mr. Horsley, of Cheltenham, have been communicated to me. In each instance a dose of strychnia, closely wrapped in paper, was administered to animals; in one, a grain was given to a cat, in the other, two grains to a dog. After death the strychnia (in the cat) was found in the stomach still in the paper wrapper,—the quantity apparently undiminished. The animals must have been poisoned by the imbibition and solution of a minute portion of strychnia through the paper. Assuming that any part of the pill remained in Cook's stomach at the time of death,—as a cut was made into the greater curvature during the inspection, it is possible that the residuary portion of pill may have escaped from the aperture unobserved with some of the fluid. Of course this is a mere speculation; but it would account for the loss of strychnia from the stomach. My belief is, that in Cook's case a small dose (but sufficient to kill) was given—that during the hour and a half which he survived after swallowing the poison, some portion (sufficient to account for death) was carried into the body by absorption, and that the residue through inadvertence, was lost during the inspection. I am

<sup>1</sup> See p. 10, *ante*.

informed by a gentleman who assisted at the inspection, that he had at the time no suspicion of poison, or more care would certainly have been taken.

The non-discovery of strychnia in the coats of Cook's stomach is therefore no great mystery; it was not owing to the failure of chemical tests, the influence of tartar emetic, putrefaction, eremacausis, or any of those occult causes which have been made the subject of newspaper speculation, but simply to the absence of the poison from that part of the body in which, if present in sufficient quantity, a chemist may have a reasonable expectation of finding it. The test of the accuracy of this view will be not in poisoning animals with doses of strychnia six times as great in proportion as the dose which would have sufficed to kill Cook, not in carefully removing the stomach without losing the contents—not in directing the entire analysis to strychnia alone, with a full knowledge that it has been taken and with a full impression that it must and will be found somewhere,—but, in the first case of a human being who may be unfortunately poisoned by a grain or a grain and a half of strychnia after it has remained an hour or longer in the living body,—in conducting the inspection as it was conducted in Cook's case, cutting the stomach from one end to the other, losing part of the contents and diffusing the remainder in a large jar containing blood, fæces and the whole of the intestinal canal. Let the viscera thus treated be placed, without any information being given, in the hands of an "adept" who professes to be able to detect the minutest fractional quantities of strychnia in a dead body, and we may then judge how far his chemical tests will bear out his statement. It is not improbable that on the discovery of one poison (antimony), diffused throughout the body, he would stop, and not direct his analysis to the research for any other.<sup>1</sup>

<sup>1</sup> The presence of poisons may be occasionally overlooked, even by chemists of great repute as analysts. In the case of *Stephens v. Barwell*, tried at the Wells Autumn Assizes, 1855, Mr. Herapath gave evidence for the plaintiff, on the composition of a certain slag found on the plaintiff's ground. He had found in it lead, zinc, silica, &c.; but when asked by Mr. Montague Smith, whether he had discovered *arsenic*, he stated that he had not looked for it. Arsenic was found in the slag by Mr. Brande, Mr. Johnson, the assayer, and myself, and its presence there had rather an important and adverse bearing on the plaintiff's case.

With this statement of the facts which have excited so much anxiety in the public mind, let the reader consider the form in which the counsel for the defence was permitted to address the jury on this important question :

“If he (John Parsons Cook) died from the poison of strychnia, he died within two hours of the administration to him of a *very strong dose*<sup>1</sup> of it,—he died within a quarter of an hour or twenty minutes of the effects of that dose being visible in the convulsions of his body : the post-mortem examination took place within six days of his death,—there is not the least reason to suppose that between the time of the injection of the poison, if poison was taken, and the paroxysm in which he died, there was any dilution of it in the stomach, or any ejection of it by vomiting. *Never therefore were circumstances more favorable ; unless the science of chemical analysis is altogether a failure for the detection of the poison of strychnia, never was there a case in which it ought to have been so easy to produce it.*”<sup>2</sup>

One other point here requires to be adverted to, since it formed a conspicuous portion of the learned counsel’s defence :

“The gentlemen who have come to the conclusion that strychnia may have been *there* (*i. e.* in Mr. Cook’s *body*, more correctly his stomach), have arrived at that conclusion by experiments of a very partial kind indeed ; they contend that

<sup>1</sup> There is not the slightest foundation for this statement. If Cook had taken a “very strong dose,” the symptoms would not probably have been delayed an hour or upwards, but would have appeared in from fifteen to twenty minutes. Further, although the term “very strong” has no definite meaning, a large quantity of strychnia could not have been contained in two small pills, such as were given by the prisoner to the deceased. The jury, however, were to be impressed with the idea that Cook’s stomach was saturated with strychnia, and therefore it ought not to have escaped the processes of the chemists, especially as the cutting up of the stomach, and the diffusion of its contents somewhere, were described to be circumstances most favorable to the analysis !

<sup>2</sup> In the case of Wooler, tried at the Durham Winter Assizes, 1855, another learned serjeant, in conducting the defence, argued strongly in favour of the innocence of the accused, on the ground that having strychnia in his possession, he would have used strychnia and not arsenic (the poison found in the body, and of which the deceased died), because “it is now pretty universally known that there is no poison so certainly detected after death as arsenic,” while to a man skilled in drugs, it was well known “that there was no poison so difficult of detection and discovery as that of strychnia.”

the poison of strychnia is of that nature, that when once it has done its fatal work and become absorbed into the system, it ceases to be the thing which it was when taken into the system; it becomes decomposed, its elements separated from each other, and therefore no longer capable of responding to the tests which, according to them, would certainly (?) detect the poison of undecomposed strychnia: that is their case. They account for the fact that it was not found, and for their still retaining the belief that it destroyed Mr. Cook, by that hypothesis."

Further, Mr. John Smith, the solicitor, in addressing Sir George Grey, with a view to a respite of the prisoner after his conviction, raised a plea upon the same ground,—“the discrepancy of the medical testimony as to the power of finding strychnia.” He observes:—

“Dr. Letheby and Mr. Herapath, two of the most eminent toxicologists of this day, upon *their solemn oaths* declared that they could discover the *fifty thousandth part* of a grain of strychnia, and that if Cook died from that poison they could now find it. Their opinions were confirmed by some of the most distinguished members of the Schools of London (?), Leeds, Edinburgh (?), and Dublin (?). Yet the body (more correctly the coats of the stomach) of Cook did not yield to the manipulations of Drs. Taylor and Rees, the smallest particle of strychnia. Since the unfortunate termination of the trial, my table has been laden with communications of scientific men (?), in confirmation of Dr. Letheby and others.”

Whether strychnia can always be found in the body of a person poisoned by it, if there be only the 1-50,000th or even the 1-1000th of a grain or less, diffused through it, is a question of fact, and not at all dependent on any theory. The extensive experience on which the answer to this question has been so conclusively settled in the affirmative by the counsel and solicitor for the prisoner, in reliance on their witnesses, will be presently examined. If a person or an animal can die from a dose of strychnia, which will leave no clear indication of its presence to the so-called chemical tests, it is practically quite unnecessary to discuss in a Court of law what becomes of the strychnia, whether it circulates through the blood in a state as permanent and unchangeable as silica (flint) or iron,

or whether it becomes decomposed and its elements separated or so combined with the animal structures as, at least in some cases, to be withdrawn from the action of chemical tests. Dr. Rees and I adopted the opinion of Liebig, who was not called for the defence (as it was previously announced he would be), because on this point, he would have probably been a most inconvenient witness to the learned counsel. The theory of the partial, not (as alleged) the complete, metamorphosis of alkaloids, such as strychnia, in the blood, is Liebig's, and not ours:<sup>1</sup> but such a question really could not be imported into Cook's case without a complete disregard of all the facts proved in the scientific evidence. In the first place, the substance of the stomach, and not the body of Cook, was examined for strychnia. It was distinctly stated, in answer to an inquiry from the learned judge, that the organs in which Sergeant Shee thought the strychnia had been theoretically decomposed were not submitted to any analysis for strychnia. Hence to argue on a theoretical cause for the disappearance of strychnia from the body of Cook, when there was no evidence to show whether it was or was not present in the parts to which it would be carried by the blood, was a palpable absurdity. It had the intended effect, however, of temporarily withdrawing the attention of the jury from the real question at issue.

There is another aspect in which this question may be viewed. Certain results have been elsewhere (*ante*, p. 60) described as having been obtained by us from the application of the tests for strychnia to the evaporated residue from the stomach of Cook. I will assume that Dr. Rees and I had taken a more sanguine view of this matter than we did, and that after hearing of the possession of strychnia by the prisoner, and reading Jones's statement, we had attached greater importance to these chemical results, and had represented them to be indicative of the presence of this poison in the body, what then would have been the effect of our evidence? The learned counsel for the defence would probably, under chemical advice, have addressed the jury somewhat in this form:

<sup>1</sup> Dr. W. B. Herapath, of Bristol, a son of the witness, has published in the 'Pharmaceutical Journal,' vol. xiii, p. 219, 1853, a useful paper showing by experiment that in the medicinal use of an alkaloid (quinine) about three fourths disappear, being assimilated or destroyed in its transit through the body!



“ Gentlemen,—I throw aside symptoms which no doubt you will agree with me, as the evidence among the medical witnesses is conflicting, may have been due to disease and not to poison, and I will at once grapple with the main, I may say the vital question, in this most important case. We are told that strychnia has been detected in the body of Cook, and two witnesses for the Crown have been called, gentlemen, I admit, of great respectability, who have sworn on their solemn oaths that they found a quantity of this subtle poison, equivalent to the thousandth of a grain in the liver of the deceased, and the one hundredth of a grain in the coats of his stomach. I endeavoured to procure from them, in cross-examination, some kind of information respecting the mode in which they had arrived at this conclusion, and what did it turn out to be? Why, gentlemen, you heard what they said—a blue, purple, and red colour, upon adding to some sort of extract obtained from the dead body of this wretched man, a mixture of bichromate of potash and oil of vitriol. I am obliged to my friend Mr. Grove, who is himself an accomplished chemist, for reminding me that bichromate of potash is a strongly coloured substance, and actually derives its name from the large variety of coloured compounds which it produces. So he tells me, and I have no reason to doubt his word. Oil of vitriol I am told, also produces colours with a great number of substances of an organic nature. Gentlemen, I have a great respect for science, but when I find two witnesses thus coming forward to swear away a man’s life upon the hundredth, aye, the thousandth of a grain of something which they suppose to be strychnia, because they noticed a little flickering blue and purple colour when they added their chemicals to it, I cannot suppress my indignation. Then again they tell us, it is true, they took extraordinary precautions to get rid of the organic matter. What, I should like to know, is strychnia but organic matter? and yet they wish to persuade you and me that they can get entirely rid of one kind of organic matter and preserve the other kind—the invisible thousandth of a grain—in a perfectly pure state, for the production of their blue and purple colours. Gentlemen, I am almost ashamed to dwell on this part of the case for the Crown. What! a man’s life to depend on the alleged detection of the thousandth of a grain of strychnia, and that, as I understand, not actually separated in a tangible form, but merely judged to be present by two learned gentlemen who were diligently looking for it, expecting it, nay, wishing it, in order to bolster up their theory of the cause of death: it is a monstrous absurdity, and a part of the evidence of my learned friend that I am sure your common sense will lead you at once to reject. But I have hardly put the case of these learned doctors fairly. One of them tells us, as some foundation for his opinion, that he could discover the fifty thousandth part of a grain, that is, unmixed with organic matter. I endeavoured to fix him to some definite quantity which he could detect when mixed with organic matter; how much he could find in a man’s heart, for instance, who had been poisoned by a grain; because I need hardly tell you, gentlemen of the jury, that the question which we have here to solve, is the discovery of strychnia in Cook’s body, which may be taken to consist of organic matter. You observed the evasiveness of his replies: he could not tell—it would depend on the nature of the matter—a small portion—a very small portion—he had no idea: and it is upon results of this doubtful kind that these witnesses for the Crown have had the incredible imprudence—an imprudence which has led to all this dreadful excitement—an imprudence which has rendered it necessary that this inquiry should take place in this

“ form and in this place, if at all—to state, upon their solemn oaths, that they found  
 “ strychnia in Cook’s body, and that Cook was poisoned by it. I need hardly tell  
 “ you that, but for this alleged discovery of strychnia in the body, there would be no  
 “ case against the prisoner.

“ But let us deal with this subject seriously. I have been long enough at the bar  
 “ to remember that gentlemen of equal skill with those who have appeared for the  
 “ Crown in this case, formerly relied in an equally positive manner on colour-tests  
 “ for the detection of arsenic. About thirty or forty years ago, whenever a yellow  
 “ or a green colour was produced by the addition of some chemical tests, the names  
 “ of which I now forget, to the liquids of a stomach, it was immediately concluded  
 “ that arsenic was there, and that the person had been poisoned; and some serious  
 “ mistakes were made in consequence. I am reminded that there have been nume-  
 “ rous false charges of poisoning by arsenic, owing to too great a reliance being  
 “ placed upon the colour-tests for this poison. And let me ask you to consider how  
 “ the case stands here with respect to strychnia, which these witnesses pretend to have  
 “ found in this extremely minute quantity in Cook’s body. On what large amount  
 “ of experience do you suppose these gentlemen to found their very positive state-  
 “ ment of the presence of strychnia in this case? Why, one tells us that he has  
 “ been occupied thirty or forty years as a chemist and toxicologist, and that he has  
 “ had only one case during that long time, in which he analysed the contents of  
 “ the stomach and found strychnia. That solitary case, gentlemen, if I understood  
 “ him correctly, was a case of suicide where a very large dose of strychnia was known  
 “ to have been taken by the deceased; and where, to help him in his analysis,  
 “ besides information given to him by the executors and a near relative of the de-  
 “ ceased, some of the strychnia was actually found wrapped in paper in the room,  
 “ or taken from the dead man’s waistcoat pocket, I forget which. The other tells  
 “ you he has been engaged in these researches for a period of fourteen years, but he  
 “ has not seen a case of death from strychnia, and therefore has never made an  
 “ analysis for the discovery of this poison in the body of a person who has died  
 “ from it. It is upon such extensive experience as this, on the first trial for  
 “ murder by strychnia which has taken place in this country, that we have two  
 “ witnesses swearing in the most positive manner that they detected, by the  
 “ use of tests, which they pretend to describe as unerring, the hundredth  
 “ part of a grain in Cook’s stomach, and the thousandth part of a grain in his  
 “ liver; and is it, let me ask you solemnly and seriously, upon evidence such as this  
 “ that the unfortunate prisoner at the bar is to be sent to the scaffold? Gentlemen,  
 “ I am inclined to think that their opinions, unsupported by the opinions of others,  
 “ cannot have much weight with you. However, what I have to say now upon that  
 “ point is, that I will call before you many gentlemen of the highest eminence  
 “ in their profession, analytical chemists, to state to you their utter renunciation of  
 “ the colour-tests, as they have been used by these chemists in their homœopathic  
 “ analyses. But, gentlemen, assuming for one moment that the witnesses for the  
 “ Crown are right, and that the substance which they found in Cook’s body was really  
 “ strychnia, of what value, let me ask, is their evidence? They both admitted to me  
 “ in cross-examination,—they could not deny it,—that the quantity of strychnia which  
 “ they found in the liver and stomach, taken together, was not sufficient to cause  
 “ death; and I am prepared to contend, notwithstanding what these men of science  
 “ tell us, and my witnesses will bear me out in the statement, that unless you dis-

“ cover a sufficient quantity of poison in a dead body to account for death, chemical analysis is altogether a failure, and it is the height of rashness to assign death to the poison. Do not misunderstand me, gentlemen; I will make an appeal to your common sense. How can a man die unless there is poison enough in his body to kill him? Talk of the thousandth and the fifty thousandth part of a grain; how could this do injury to a person, when a much larger quantity is given for a medicinal dose, and we are told by these learned doctors that less than half a grain of strychnia has never been known to destroy life? Besides, admitting it to be strychnia, their science does not even allow them to tell you how long it has been there—whether days, weeks, or months—and, for anything they or you or I know, it may have been the result of the judicious or injudicious use of strychnia at some former period of this unfortunate young man’s career; in fact, long before he had ever seen or heard of the prisoner at the bar. I have already had to comment on the question of the antimony found in Cook’s body. I attached no importance to that, because half a grain, the quantity found, is admitted to be really harmless, and the evidence is, therefore, not of the slightest importance in the case. If we reject the half grain of antimony as unimportant, how can we attach any importance to the presence of the hundredth or of the thousandth of a grain of strychnia. The witnesses for the Crown pretend to account for the small quantity found in the body by the effect of vomiting, absorption, decomposition in the blood, elimination, and other fine theories which no one but themselves can understand. But, gentlemen, if Cook was poisoned at all, he must have been poisoned by a very strong dose—many grains; and can you suppose that in the short space of two hours there would have been no more strychnia left in his stomach and liver than these chemists profess to have discovered? The eminent gentlemen I intend to call will tell you that such theories as these, which they have heard for the first time in this Court, are utterly unworthy of credence. Away then with the colour-tests; let the witnesses produce from Cook’s body a quantity of strychnia which will alone satisfy men of your intelligence that it was sufficient to cause death,—let them produce at least half a grain of strychnia, or their chemical tests are of no value to us. In a memorable case of poisoning by prussic acid, which was the subject of a trial for murder some years since, one grain of this poison was stated to have been found in the stomach; and it was very properly contended by scientific men engaged for the defence, that, as up to that time no instance had occurred of a person having died from so small a dose, the deceased could not have died from it. Gentlemen, if a grain of this powerful poison may be found in a dead body, and persons of high scientific attainments assure you, upon their solemn oaths, that they attach no importance to it, because the quantity is or was insufficient to cause death, how can you for one moment attach any importance to the evidence of these chemical witnesses who, for the first time in their lives, profess to have extracted their thousandths and fifty thousandths of a grain of strychnia from the solids and fluids of a dead body.”

I entertain no doubt that had Dr. Rees and I relied upon such precarious results as the colour-tests gave to us in this case, this would have been the kind of defence; and objections would have been very properly taken to our evidence. There

is no reason why one who has sufficient reliance upon his chemical processes should not swear to the presence of a thousandth or a fifty thousandth of a grain of strychnia in a dead body; different degrees of evidence satisfy different minds, and we felt that we had the right to exercise that independent judgment which we should have conceded to others in a similar case. That judgment we exercised in excluding altogether results which did not appear to us to be satisfactory on a question so momentous as this. Had we taken a different course, there is not the least doubt that the strong part of the case, namely, the evidence from symptoms, would have been thrown aside, and a direct and successful attack made upon the chemical evidence. Had the results been even much more marked than they were, this evidence could not in my opinion have maintained its ground against the objections that might have been fairly urged against it. The strength of evidence in Cook's case consisted, medically speaking, in the nature, progress, and duration of the symptoms. Whether the chemical evidence would or would not have been stronger had the stomach been sent in a proper and unmutilated state, is entirely another question,—one which is now beyond the reach of solution.

When so much stress was laid at this trial upon the facility of detecting strychnia in cases of poisoning by it, it becomes desirable to examine this question by reference to past experience, and the reader will probably expect to find, from the very positive statements made by the chemists for the defence, that there is already a large accumulation of facts under this head in the human subject. But it is not so. Deaths from strychnia have hitherto been comparatively rare in this country, and the cases in which the poison has been found in the stomach, prior to the occurrence of the case of Cook, very few. Of its discovery in the absorbed state,—in the blood, or deposited in the organs, there was not, up to the time of the occurrence of this case, a single instance reported. The principal medico-legal journal of France, the '*Annales d'Hygiène Publique et de Médecine Legale,*' which records all cases of poisoning of interest that occur in that country, —does not contain the report of any instance of poisoning by strychnia since its establishment in 1829, during

a period of twenty-seven years. In Henke's 'Zeitschrift für die Staatsarzneikunde' there is an equal dearth of facts,—I cannot find that one case has been reported during a period of eighteen years. In Casper's 'Wochenschrift für die gesammte Heilkunde' there is a report of a case which occurred in 1846. The details of this case are given in No. 7 in the subjoined list. Our own British journals, extending over a series of years, have contained the reports of but very few cases. It is possible that some may have been overlooked; but all that I have been able to find by diligent search or by communication with medical friends are described in the table. There have been many instances in hospital and private practice, in which symptoms of poisoning have followed the accidental administration of strychnia prescribed medicinally; but as the patients recovered, these will not assist us in the present inquiry. The case No. 10 in the table led to a trial for manslaughter. So far as it is known, there have been three murders by strychnia in this country since its introduction about the year 1820. I allude to the cases of Miss Abercromby in 1830, of J. P. Cook in November 1855, and of Mrs. Dove in March 1856. I have also the particulars of three cases which have given rise to trials for murder in the United States and Canada.

The reader will understand, therefore, that in the subjoined table, the cases are limited to those in which strychnia has destroyed life, and in which the facts are recorded in an authentic form. The *symptoms* and *appearances* in each case are tabulated so as to admit of comparison. It will be instructive to compare these with the objections taken in the defence of William Palmer by some of the medical witnesses. It will be perceived, as I have elsewhere had occasion to state, that the circumstances on which reliance was placed to show that Cook had *not* died from strychnia, have presented themselves repeatedly in cases in which death from strychnia could not be disputed. The publication of this table may be of great value as a guide to medical witnesses in future cases.

TABLE OF CASES OF POISONING BY STRYCHNIA.

No.	Authority and Date of Occurrence.	Dose taken and Symptoms observed. Period of Death.	Post-mortem Appearances.	Analysis and Results.
1	<p>Case in Hospital practice, 1831, Dr. Booth and Dr. Bardsley. 'Trans. Prov. Assoc.,' 1834, ii, 215; 'Med. Times and Gazette,' July, 1856. A man, <i>æt.</i> 46.</p>	<p>One grain and a half taken at a dose, after repeated smaller medicinal doses. Stupor; loss of speech; tetanic convulsions of the whole muscular system; scalp; vessels of dura mater turgid opisthotonos so severe, that but for assistants, patient would have been thrown off the bed; consciousness retained; breathing short, laborious, stertorous; pupils dilated; insensible; slight intermissions of convulsions; tetanic spasms more and more violent; gasping breathing; body rigidly and permanently bent backwards; cold sweats. Death in <i>two hours</i> and <i>three quarters</i> after having taken the poison.</p>	<p>Seven hours after death. Fingers firmly contracted; muscular system generally rigid; blood on dividing vessels of dura mater turgid with dark-coloured blood; arachnoid opaque, and thickened. In right corpus striatum, an apoplectic clot; brain softened around; serous effusion in ventricles; other parts of brain healthy; spinal membranes highly vascular; pia mater had a florid redness, and congested with arterial blood; four patches of extravasated blood between this membrane and arachnoid, opposite last dorsal and upper lumbar vertebra; spinal marrow healthy; chest and abdomen not examined.</p>	<p>No analysis.</p>
2	<p>Case recorded by Dr. Blumhardt, reported in Wibmer's 'Arzneimittellehre,' art. 'Strychnia,' p. 254, 1839. Also 'Med. Corr. Blatt des Wurtemb. Vereins.,' 1837, and 'Lancet,' Jan. 7, 1838. A young man, <i>æt.</i> 17</p>	<p>Forty grains (2 scrupel reines strychnin) of pure strychnia taken in a glass of water. Soon began to feel great anxiety and restlessness. Four grains of tartar emetic given but slight vomiting. In <i>fifteen minutes</i> after taking the poison, he was stretched on his back, on the bed; countenance</p>	<p>Inspection made twenty hours after death. In spite of great heat of weather, body unusually stiff and rigid; no trace of putrefaction; muscles universally of a brownish-red colour. On view of the spinal canal, two pounds of tarry blood of tarry consistency escaped; vessels of the spinal mem-</p>	<p>As so large a dose of strychnia had been taken, analysis was made of the contents of the stomach, with a view of determining whether any part of the poison could be chemically demonstrated to be present; <i>but there was</i></p>

<p>anxious; head and body stiff. Membranes congested; spinal marrow at <i>not a trace of strychnia</i> to be wished to turn on his right side, but the upper part soft, at the lower, hard; <i>found</i>. The blood collected could not, having no power, except general congestion of the brain and its retained its thick fluid consistency, and did not undergo over the upper extremities. Had his membranes, with dark fluid blood; sistency, and did not undergo full consciousness, and spoke in a loud cerebellum softer than natural. Viscera putrefaction. It was exatone, in his usual manner, on his con- of abdomen and thorax deficient in mined for strychnia, but no trace was found.</p> <p>dition, occasionally interrupted by a slight stiffness of the jaw; blood, as if this fluid had been forced breathing became difficult; pulse small and quick; occasional fits of into the central organs of the nervous system, involving the jaw and the whole body. With a violent shud- system. Lungs healthy, distended with air, and bloodless; heart spasm of all the muscles of his body, became as stiff and rigid as a flabby, and <i>empty</i>, as well as the great vessels near it, rendering statue; arms spasmodically bent at the elbows, and drawn over the it difficult to collect any for chemical examination. The blood every- chest; feet distorted, and soles inclined to each other; retention where fluid. The stomach was full of fresh, solid food; no appear- of consciousness; face and lips livid; eyes protruding, pupils dilated; ance of the poison; mucous membrane about the cardia reddened. moaning; intervals of relaxation. Crushed the tube of stomach- The liver contained more blood than the other abdominal viscera. pump between his teeth. Death in <i>one hour and a half</i> after taking Gall-bladder empty.</p>	<p>Case by Mr. Bennett, occur- <i>One grain and a half of strychnia,</i> red at Sydney, April, 1838. taken in solution, on an empty stomach. 'Lancet,' August, 31, 1850. Symptoms began by twitchings, rather more than <i>an hour after taking the poison</i>; general tremors; limbs rigid; whole body stiffened and straightened; neck drawn back; eyes protruding; pulse imperceptible; face livid; froth from mouth; violent tetanic convul- sions, with opisthotonos; hands clenched; arms bent; legs and body ex- tended. She died in a violent fit, <i>two hours and a half</i> after taking the poison.</p>	<p>No inspection.</p> <p>No analysis.</p>
<p>4</p> <p>Dr. Watson, Glasgow, Sep- tember, 1845, 'Edinburgh Monthly Journal,' Dec. 1845, p. 908. A girl, æt. 12 (case quoted at the trial of W. Palmer).</p>	<p><i>Three quarters of a grain of strychnia.</i> Symptoms came on suddenly in twenty minutes; arms extended and rigid; face flushed; lips livid; jaw not fixed; body curved, opisthotonos. Death in three quarters of an hour from commencement of symptoms; al- together a little over <i>an hour</i>.</p>	<p>Forty-four hours after death. Brain and membranes natural; turgescence of velum and choroid; skin of head bled freely; <i>spinal cord healthy</i>, membranes rather vascular; lungs con- gested; muscular fibre of heart stiff; <i>cavities of heart quite empty</i>; mucous membrane of stomach pale and natural.</p> <p>No analysis.</p>

No.	Authority and Date of Occurrence.	Dose taken and Symptoms observed. Period of Death.	Post-mortem Appearances.	Analysis and Results.
5	J. C. Evans, November 17, 1845. 'Medical Gazette,' vol. xxxvii, 1846, p. 925. A man, æt. 26.	Dose and symptoms unknown. Found dead. Body and extremities quite stiff; nails imbedded in palms of hands; pupils dilated; eyeballs protruded; gorged; heart filled with coagulated dark frothy saliva from mouth; teeth blood; stomach contained a dark greenish-looking substance like coffee grounds.	Thirty-six hours after death. Vesicles of head turgid; brain and membranes healthy; lungs healthy, but very bitter, milky looking, but cleared by ammonia. <i>Sulphuric acid gave a beautiful rose-pink colour.</i>	Stomach and contents boiled in water; liquid evaporated, but very bitter, milky looking, but cleared by ammonia. <i>Sulphuric acid gave a beautiful rose-pink colour.</i>
6	January, 1846. Cormack's 'Monthly Journal,' Feb., 1846, p. 141; 'Medical Gazette,' vol. xxxvii, p. 254. A woman, æt. 35.	<i>Three grains of strychnia</i> , taken in powder in tea. A flow of saliva; no complaint of taste. In twenty minutes, convulsions, every limb shaking; short intervals of recovery, in which she uttered exclamations expressive of great pain; the least motion produced another paroxysm; shaking so powerful that it required several persons to hold her. Death, <i>one hour and three quarters</i> after taking the powder.	No inspection.	No analysis.
7	Case of a Medical Student, reported by Dr. Theinhardt, Casper's 'Wochen-schrift,' February 28, 1846, p. 143.	About <i>thirty grains of strychnia</i> , taken in spirit; seen in one quarter of an hour afterwards; breathing, pulse, and appearance natural. Spasms in the muscles soon commenced, with hurried respiration; twitchings, followed by perfect rigidity of the whole body. An emetic given without effect. In a few minutes, another paroxysm, with violent motions of the whole body, and opisthotonos. A third and fourth paroxysm followed, the patient groaning and screaming. He died in <i>half</i>	Tongue, gums, and lips, violet-colored, as well as the fingers and toes; the hands were clenched, and the toes drawn backwards; the whole body stiff and hard, like a piece of wood, bent somewhat backwards. No inspection.	No analysis.



<p>8</p>	<p>Dr. Edwards. 'Edinburgh Monthly Journal,' No. 64, April, 1846, p. 230. An adult man.</p>	<p>Dose and symptoms unknown. Body lying on back, in bed, rigid, and stretched out to its full extent; eyeballs prominent; pupils dilated; mouth firmly shut; arms lying over chest; hands firmly clenched; lower extremities particularly rigid, and left foot concave.</p>	<p>Twenty-eight hours after death. Decomposition commenced; partial relaxation of joints. Brain and membranes healthy, but general turgescence of vessels; lungs congested with dark fluid blood; some tubercular disease; heart dilated, but with vessels healthy, contained a quantity of dark fluid blood; spinal marrow not examined; stomach, intestines, and other viscera healthy.</p>	<p>A bottle labelled poison, and containing strychnia, found near the body. Stomach and contents boiled half an hour in vinegar, and filtered: ammonia gave a fine white precipitate; converted to pale red by nitric acid; sesquichloride of iron a pale green; tincture of galls an adhesive precipitate. From these experiments concluded to be strychnia.</p>
<p>9</p>	<p>Case of Dr. Warner, October 11, 1846. 'British American Journal,' August, 1847. An adult, æt. 39.</p>	<p>Half a grain of sulphate of strychnia, taken by mistake for sulph. morphia. Symptoms began in less than five minutes; constriction of throat; tightness of chest; rigidity of muscles, in attempting to move; first complained of want of air, and requested the windows to be raised; violent tetanic convulsions, opisthotonos, lasted five minutes, succeeded by partial calm; livid countenance; frothy matter from mouth; moans; attempts to vomit. Death in from fourteen to twenty minutes; mind clear until the last!</p>	<p>No report of examination of body.</p>	<p>No analysis.</p>

Dr. Warner's case is usually considered to represent the smallest dose of strychnia which has proved fatal to an adult. A case occurred in a London hospital, in October, 1853, in which it seems probable that a *quarter of a grain* of strychnia in one drachm of water, at one dose, destroyed the life of a woman, aged 36. The patient was affected with trismus, as the result of a burn, and had been treated with opium. The muscular system was relaxed, and the breathing was short and spasmodic. In ten minutes after the strychnia had been given, the respirations were increased, and almost immediately after the patient was seized with the most violent convulsions, the back being bent forwards (emprostotonos), the arms raised, and the features contorted. After a time, these spasms ceased for the space of about a minute: they then recurred with extreme violence, and continued until death took place, in about an hour and three quarters. (See 'Med. Times and Gazette,' April 15, 1854, p. 376.) A patient of M. Andral suffered from the most violent tetanic convulsions from so small a dose as *one twelfth of a grain* of strychnia. There are probably some constitutions peculiarly susceptible of the effects of this poison. (See Appendix, on fatal dose, p. 138.)

No.	Authority and Date of Occurrence.	Dose taken and Symptoms observed. Period of Death.	Post-mortem Appearances.	Analysis and Results.
10	Case of Mrs. S. Smyth. 'Pharm. Journal,' 1848, vol. ii, p. 298. An adult female (case quoted at the trial of W. Palmer).	<i>Three grains of strychnia</i> , in liquid, by mistake for salicine. Spasms in from <i>five to ten minutes</i> ; screaming; legs drawn up; feet turned inwards; body stiff; easy before death; said "turn me over;" died tranquilly; consciousness retained. Death <i>one hour and a quarter</i> from taking the medicine.	Inspection three days after death. Body healthy; contraction in feet continued, but had gone off somewhat from the rest of the body; no disease in any part; heart contracted, and perfectly empty. The blood was fluid.	No chemical analysis of stomach or contents. (Medicine analysed.)
11	Case in Provincial Hospital, in 1848. 'Med. Times and Gazette,' April 28, 1855. A girl, <i>æt. 12</i> .	<i>One grain of pure crystallized strychnia in pill</i> . Soon afterwards, there was violent sickness; convulsive spasms of a tetanic character followed. She died in <i>one hour and a half</i> .	No report of inspection. Death referred to some undiscovered disease of nervous system — when, four years afterwards, it was found that the bottle labelled " <i>Valerianate of Zinc</i> " from which the pill had been prepared, contained pure <i>strychnia</i> !	No report of analysis.
12	Case reported by Dr. Smith, of Forfar, occurred in May, 1851. 'Edinburgh Monthly Journal,' September, 1851, p. 234. An adult male.	Dose and symptoms unknown. A game-keeper, found dead; a phial labelled strychnia, poison, containing white powder, lying near; deceased had not been seen for four days.	Body lying on face; hands firmly clenched; feet much extended; head considerably bent backwards; every joint rigid, and some muscles had the condition as if in powerful action during life; countenance livid, and somewhat swollen; dark-coloured fluid blood-tinged; bloody points in brain; choroid turgid; heart natural, white sediment. The powder in phial gave similar results. Contents of stomach subsequently examined by Dr. Christison, concentrated to a thin syrup, and filtered again. The mixture dried at 212°, treated with boiling rectified spirit, and evaporated to dryness. Extract had intense bitter taste of strychnia, and turned bright yellow by active acid. The colour-test did not act characteristically. Dr. Christison remarks, "There is, in fact, a want of positive tests for strychnia when mixed with organic matters, unless the proportion were considerably greater than in this case, so as to admit of some complexity of chemical handling. I think there is satisfactory evidence for a case of suicide or accident. It may be doubted whether this chemical evidence, however, would be enough on a charge of murder, in which there was no proof of the nature of the symptoms."	Stomach contained no food. Only a few ounces of fluid exhaled; a strong spirituous odour; portion of contents boiled with dilute sulphuric acid; nitric acid added produced a light orange; tannin caused a deposit of a gray-white sediment. The powder in phial gave similar results. Contents of stomach subsequently examined by Dr. Christison.

<p>13</p> <p>Case reported by Dr. Lonsdale, occurred November 28, 1854. 'Edin. Monthly Journal,' 1855.</p> <p>A strong man, æt 59.</p> <p>of whole body, which was most marked in back and legs; in the thorax; violent tetanus respiration, in which state he of taking the poison. Deceased tellect perfectly clear; pupils fixed; face and hands livid; arms were unnaturally rigid; rasp the palm.</p> <p>an alcoholic solution of the stomach.</p>	<p>Inspection thirty-six hours after death. Brain and medulla in natural state; heart healthy, empty, and unnaturally atonic; stomach and bowels congested; slight congestion of left kidney.</p> <p>acidulated with sulphuric acid; evaporated to dryness, and filtered, again filtered, and filtrate evaporated; a slight yellowish colour, and a bitter taste. To the extract, sulphuric acid and bichromate of potash added, gave a purplish tint, speedily changing to a permanent light green. This test, as well as the peroxide of lead and sulphuric acid, failed to give any appreciable change of colour in a small quantity of the extract. Sulphuric acid and bichromate of potash added to the extract, like the effect on the extract from</p>
<p>14</p> <p>Case reported by Mr. D. G. M'Pherson, occurred at Bristol, in November, 1854. 'Medical Times and Gazette,' December 16, 1854.</p> <p>A male adult.</p> <p>membrane of trachea much congested, with black fluid blood in left ventricle; chromate of potash, gave the purple colour for a few seconds, and the stomach and intestines were of a darkish buff colour.</p>	<p>Inspection eighteen hours after death. Abdomen warm, tense, and hard; lying on its back, rigid; teeth clenched; eyes a dull, fishy appearance; flexed, fists clenched; bloody fluid issuing from the mouth. Stomach and duodenum presented red spots of extravasation (congestion?); mucous membrane softened. Both lungs engorged; presenting black patches of pulmonary apoplexy; mucous surface. Heart flabby; an ounce of spinal marrow not examined.</p> <p>In deceased's box a phial was found labelled "Poison, strychnine, 10 grains;" and on the dressing-table was a glass with some broken crystals which had the bitter taste of strychnia, and when tested by bichromate of potash and sulphuric acid gave the purple colour of strychnia. The stomach contained about two ounces of thick fluid, in which were a few grains that, with the sulphuric acid and the bi-</p>

No.	Authority and Date of Occurrence.	Dose taken, and Symptoms observed. Period of Death.	Post-mortem Appearances.	Analysis and Results.
15	<p>Case reported by Dr. Geoghagan, in 'Dublin Medical Press', occurred in July, 1855.</p> <p>A male, æt. 26.</p> <p>[A report of this case was not published until the 25th of June, 1856. (See the 'Dublin Medical Press' of that date.)]</p>	<p><i>Five grains of strychnia.</i> Heard moaning in <i>fifteen minutes</i>; lying on back in bed; hands clenched; fore-arms flexed; one leg extended, the other in the act of being so convulsively; head drawn back. He died in <i>twenty to twenty-five minutes</i> from taking the poison.</p>	<p>Inspection eighteen hours after death. Body fresh, rigid, resting chiefly on back of head and heels; limbs bent with difficulty; feet extended; face calm; mouth slightly open, with brown matter on tongue, and froth of same colour adhering to the teeth; brain natural, its vessels containing but little blood; lungs moderately congested; the heart was firmly contracted, its cavities contained a very small quantity of dark fluid blood; mucous membrane of stomach softened, and presenting dark clay-brown patches; subjacent veins injected; contents partly digested food of a red-brown colour, acid reaction.</p>	<p>Filtered contents of stomach were strongly bitter. Contents (acidulated?) agitated with animal charcoal to remove the strychnia; this afterwards removed from charcoal by boiling rectified spirit; evaporated; dissolved in liquor potassæ, and agitated with ether by Stas's process. The ethereal extract leaves the strychnia sufficiently pure for testing, or the conversion of residue into acetate of strychnia, and a repetition of the potash and ether. <i>a.</i> Alkaline reaction and bitter taste of ethereal extract. <i>b.</i> Marked</p>
16	<p>Case of John Parsons Cook; date November 20th, 1855. Et. 28.</p>	<p>Dose taken unknown. Symptoms on Monday night, the 19th, about an hour and a quarter after some pills had been given by Palmer; violent screaming first heard; beating of bed-clothes with his arms; complained of feeling of suffocation; his head drawn back; moving, as by jumping or jerking, both</p>	<p>First examination six days after death. After death, body rigid, and hands clenched; feet turned inwards; abdominal viscera healthy; stomach contained some ounces of a brown fluid, at cardiac extremity some congestion, white specks on the mucous membrane; small intestines</p>	<p>Parts examined for strychnia;—coats of stomach, and two ounces of bloody fluid from the jar containing it. Heated with dilute sulphuric acid and neutralized by carb. potash, evaporated, and digested in al-</p>

in the head, and all over the body; contained a bilious fluid; kidneys con-  
 tained alcohol; evapo-  
 difficult breathing; eyes projecting; gested, right larger than the left, rated, and tested by taste, as  
 gasped when he spoke, as if difficult larynx stained with dark blood; lungs well as by sulphuric acid and  
 for him; hands fixed and stiff, asked contained much fluid blood, accounted bichromate of potash, a slight  
 to be rubbed; twitchings in the arms for by gravitation; heart natural size, purple colour produced, but  
 and body during the rubbing; twitched and healthy, its cavities were empty; no satisfactory evidence of  
 all over; quite conscious; all the time blood throughout body fluid, and un- strychnia. Antimony found  
 recognised Palmer, and said "Oh, Pal- coagulated; brain and spinal marrow in these parts. Three drachms  
 mer, I shall die;" had a difficulty in healthy; dura mater of brain con- of blood (the whole quantity  
 swallowing some pills which were given gested. sent), antimony found there-  
 to him. In taking some liquid, he Second examination two months in. Liver, spleen, kidneys,  
 snapped at the spoon, and seemed to after death. Body and limbs still very and lungs, examined only for  
 bite it very hard, his body was then rigid; hands closed firmly; putrefaction mineral poison. Antimony  
 jerking and jumping; snapped in same not much advanced; spinal cord and alone found in small quantity,  
 way at a glass containing a draught canal examined, both quite healthy. in all the parts submitted to  
 which was given to him by Palmer. examination.  
 When swallowed, it was vomited imme-  
 diately, no pills could be found; fit lasted half an hour, then became composed (Mills's evidence.)  
 Twenty-four hours after this attack, from which he perfectly recovered, seized with another and more  
 severe attack, about an hour after taking two pills also given to him by Palmer; started up; stiffness  
 of neck, which he asked to have rubbed; swallowed two other pills (ammonia?); uttered loud screams;  
 dreadfully convulsed in all the muscles of the body; opisthotonos; complained of suffocation; asked to be  
 lifted up and turned over; could not be done owing to rigidity of the whole body; quite conscious; heart  
 gradually ceased, and he died tranquilly, about *one hour and a quarter* after taking the pills.

In examining this table of deaths from strychnia, it will be perceived that of the fifteen fatal cases which I have been able to collect by searching authentic records, extending over a period of nineteen years, eleven only, prior to the case of Cook, have occurred in this country. In seven of these cases no analysis was made. In Dr. Blumhardt's case (No 2), although so much as two scruples had been taken, and the patient died in one hour and a half, no strychnia could be found either in the stomach in an unabsorbed, or in the blood in an absorbed state. Although the colour-tests for strychnia were not then known yet the most delicate test for this poison, namely the *bitter taste*, was accessible to the analyst. In fact, according to the recent observation of Dr. Christison, the absence of bitterness is the best negative test of the absence of strychnia. The case is further remarkable from the fact, that although one of the oldest authentic cases on record, there has been none in which the symptoms and appearances have been more accurately or minutely recorded. It appears from this history, that the patient, a young man, must have taken a much larger dose than Cook, and he died from its effects in about the same period of time ; yet, although a special research for the poison was made by a competent person, who had access to the stomach and contents in an unmutated condition, *not a trace of strychnia was found in the stomach, in its contents, or in the blood.* In case No. 5, except by the bitter taste, there was no evidence of strychnia, and the sulphuric acid is stated to have produced a rose pink colour, without any addition of bichromate of potash. The effects were such as would be produced by salicine and not by strychnia. The tests applied, in the case No. 8, cannot be said to have demonstrated the presence of strychnia in the stomach. In case No. 12, examined by Dr. Christison, the tests applied are tests for brucia, and not for strychnia. Although the man had evidently died from a large dose of strychnia, the colour-test did not reveal this, and it is properly stated that such chemical results as these would be of no value, unless there was *proof from the nature of the symptoms.* In No. 13, the colour-tests were applied to the stomach and contents, and gave some, but not conclusive, evidence of the presence of the strychnia. Strychnia is peculiar in not rapidly reducing chromic acid to the state of green oxide of chrome, and

yet this appears to have been the most striking and prominent colour produced, a result owing to the presence of alcohol or organic matter, and probably the high temperature, caused by the admixture of sulphuric acid. No. 14 is the only case reported to have occurred at Bristol, which I have met with, and it is probably the case referred to by Mr. Herapath, in his evidence. Strychnia was here found in the contents of the stomach by the colour-tests, but the presence of organic matter appears to have affected the results. The case presented no difficulty. A large dose of the poison had been taken—some crystals were left in a drinking glass—the stomach and contents were properly preserved—and there was no poison but strychnia to engage the researches of the analyst. No. 15, the case reported by Dr. Geoghegan, as having occurred in July, 1855, has been published by this gentleman since the case of Cook has drawn general attention to the subject; it is the only one in the table in which strychnia was clearly and unambiguously detected in the stomach, not merely by the colour-test, but by those other corroborative characters which render the demonstration of the poison conclusive and satisfactory. It was found in the contents of the stomach and in the coats of that organ; but, as Dr. Geoghegan properly remarks, this must not be regarded as absorbed strychnia, but rather as imbibed from the fluid contents of the stomach. In forming an opinion regarding an analysis, however, it is always necessary to consider the circumstances attending the case. In Dr. Geoghegan's case, the large dose of *five grains* was taken,—there was no vomiting, and the person was dead in *twenty-five* minutes. This gentleman also had, beyond doubt, the contents of the stomach for the purpose of his analysis.

I have records of many other cases of poisoning by strychnia, in which persons have recovered, and one case which has occurred since that of Palmer, in which I am informed that strychnia was detected both in the contents of the stomach and in the blood. The cases of recovery, with one exception, do not throw any light on the question we are now considering, namely, the detection of strychnia in the body. The exceptional case is, however, too remarkable to be altogether passed over. It occurred to a patient in St. Bartholomew's Hospital: the analysis was made by Dr. Stenhouse, Professor of

Chemistry in that Institution, well known for his researches in organic chemistry. A man having taken strychnia medicinally, attempted suicide by swallowing at once *four grains*, mixed with four grains of morphia, dissolved in one ounce of spirit. The usual symptoms appeared in rather more than half an hour. He was brought to the hospital *one hour* after he had swallowed the poison; and by the aid of the stomach-pump, three or four ounces of animal charcoal mixed with water, were injected as an antidote. "The stomach was completely emptied, and during the whole time occupied by this proceeding (twenty minutes), he had no convulsion." The contents thus withdrawn, under the most favorable circumstances, were examined by Dr. Stenhouse, but we are informed "he did not detect any of the strychnia." The patient, it appears, had short paroxysms at intervals after the removal of the contents of the stomach, showing that his system was still under its influence, probably owing to the portion of poison which had been absorbed. The last convulsion occurred three hours after his admission. From that time the recovery of the patient was complete.<sup>1</sup> This case merits a little consideration in reference to the alleged certainty of detecting strychnia in the stomach. It presents us with a simple fact and not a mere speculation. The analysis was performed by a gentleman who has devoted more attention than most chemists in London or the provinces, to organic analysis, and the very means were used,—animal charcoal—for fixing the strychnia, which are recommended by Graham and Hofmann as the most certain and satisfactory. In fact, they were such as many analysts of experience would have employed, supposing the man to have died at the time the stomach-pump was introduced. Nothing is more easy, as experiment will show, than to procure strychnia from animal charcoal thus employed, by the use of rectified spirit; but, although, *four grains* of strychnia had been swallowed, and only an *hour and twenty minutes* had elapsed, not a trace of the poison could be detected in the contents of the stomach. Is it to be supposed that Dr. Stenhouse would have been more successful, in this instance, had he operated on these contents in the dead body in place of the living? I see no reason for coming to this conclusion, and, whatever theory we may advance to account for its

<sup>1</sup> Report of the Abernethian Society, 'Medical Times and Gazette,' April 28, 1855, p. 423.



disappearance, whether by absorption or decomposition, the fact must be taken to prove conclusively, that a large dose of strychnia may in some instances entirely disappear from the stomach in little more than an hour. It may appear surprising that the man should have recovered at all after having taken so large a dose, but it is to be observed that the medicinal dose of strychnia which he had been in the habit of taking for two weeks, had reached half a grain. A report of this case was read before the Abernethian Society, in April, 1855,—a Society which adopted the unusual course of prematurely criticising and condemning on newspaper reports, the evidence of the medical witnesses for the Crown, in the case of Palmer. Their own records would have shown them, that in one material point at least, the medical witnesses were right, and they were wrong; and the verdict of the jury should teach them caution in the future discussion of such matters.

If we refer to toxicological authorities we shall find that there is not one who supports the statement of the chemists for the defence, that, in cases of poisoning, strychnia is not only easily detected in the stomach, but that no conceivable case of poisoning can occur by it in which its presence cannot be determined in the dead body by chemical tests. Dr. Christison, in the latest edition of his work, gives no special process for detecting strychnia, but, under the head of *nux vomica*, observes, "Contrary to what takes place in regard to vegetable poisons generally, *nux vomica* is often found in the stomachs of those poisoned with it."<sup>1</sup> He does not refer to any case in which

<sup>1</sup> 'Treatise on Poisons,' p. 896. The woody fibre of *nux vomica* protects it from the solvent or absorbent action of the stomach, and it thus remains adhering to the mucous membrane, holding a portion of strychnia which a chemist can easily separate and detect, when the strychnia itself, in an independent state, would have been long since absorbed and removed. It is difficult to understand how, in spite of this well-known fact, Mr. Herapath, by his evidence, should have tried to impress the Court with the idea that there is greater difficulty in detecting *nux vomica* than in detecting strychnia. In answer to Lord Campbell, he said, "I can extract the strychnia from the body, *even* where *nux vomica* has been the cause of death." *Ques.* "Do you mean that *nux vomica* is more difficult to discover?" *Ans.* "It is more *complicated* of course." There is no complication in the matter. *Nux vomica* is quite insoluble in all organic liquids, and is not absorbed; the powder, if present, may be easily obtained from the stomach by washing and subsidence. When thus obtained, there is no chemical difficulty in obtaining from a sufficient quantity of it, clear evidence of the presence of strychnia and brucia.

pure strychnia or its salts, taken as poisons, have been found after death in the stomach or tissues.

The counsel for the defence, in referring to Orfila on a point on which Orfila's views were supposed to be favorable to his case, properly described him as having occupied the highest rank among analytical chemists (toxicologists). Orfila, in the latest edition of his '*Traité de Toxicologie*'<sup>1</sup> (1852), revised shortly before his death, gives no case of poisoning in which he has found strychnia in the dead body. Four experiments are related, two performed by himself, and two by others. The experiments consisted in adding small quantities of the sulphate and acetate of strychnia to various mixtures of animal and vegetable matters, allowing them to ferment or decompose during a period varying from a few days to three months, and then applying tests to the filtered decoctions of these mixtures. These tests consisted, not in the use of the "colour-tests," which Orfila does not mention, but in the application of taste to detect bitterness, and in the use of nitric acid to determine the presence of brucia. The results, as far as they went, showed that strychnia is not liable to be entirely removed or destroyed by putrefaction. But in these experiments the tests acted so dubiously, that Orfila felt himself compelled to make the following observations :

"On voit par ces expériences, que s'il est possible de déceler la strychnine ou ses sels au milieu de liquides organiques colorés, il est néanmoins difficile de constater quelquefois, l'ensemble de leurs caractères : *on ne saurait donc être assez circonspect lorsqu'il s'agira de se prononcer sur un empoisonnement par cet alcaloïde, et il faudra surtout tenir grand compte du commémoratif et des symptômes éprouvés par le malade.*

"Ici comme dans l'empoisonnement par les sels de morphine et de brucine, *il ne suffit pas de s'attacher à des phénomènes de coloration : il faut, pour établir l'existence du poison, mettre à nu la strychnine ou ses sels, de manière qu'on puisse constater tous leurs caractères.*" (Tome ii, pp. 595-6.)

It will be seen, therefore, that there is not a single fact in Orfila's treatise to justify the statement that strychnia is, and

<sup>1</sup> Tome ii, p. 594, Paris, 1852, cinquième édition.

must be, uniformly discovered in every case of poisoning. The adding of strychnia to dead organic liquids and solids, and detecting its presence at different periods subsequently, is one thing,—the searching for it in a dead stomach in which it has been subjected for one or more hours to living processes, is another. Absorption and other vital processes, do not go on in porcelain vessels or earthen jars, as in the living stomach; and the results of such experiments cannot, therefore, throw any light upon the period at which, if introduced during life, we may expect to find the poison in the dead body. Besides, the quantity of salts of strychnia upon which Orfila operated in such a prepared mixture, was not the fifty thousandth, or even the one thousandth of a grain, but thirty centigrammes, or nearly *five grains*! Even when operating on such a quantity of the poison, the reader will mark the caution and circumspection with which this deservedly great toxicologist speaks of the inferences to be drawn from his chemical researches. In the cross-examination of Dr. Rees by Mr. Grove, who has deservedly the reputation of a scientific man and a good chemist, this gentleman, with the work of Orfila before him, put the following questions to the witness:

“ Q. You have told us that you consider the poison must be absorbed,—do you know that *when absorbed* it has been found in the blood and tissues? A. I do not know of any satisfactory experiment to that effect. Q. Do you know that Orfila has found it in matter that has been putrefied for a long time? A. I am not aware of it: I do not think of necessity that putrefaction would destroy it: it may. Q. In Mr. Cook’s case putrefaction had hardly set in? A. We had the body approaching the condition, but there was no very marked degree of decomposition.”

The answers given by Dr. Rees were perfectly consistent and proper. But what can be said of a series of questions of this kind emanating from a scientific man? The second question, in reference to the discovery of strychnia by Orfila in putrefied matter, is ingeniously made to follow one in which the counsel had asked the witness whether, when the poison had been *absorbed*, it had been found in the blood and tissues. On the witness replying that he knew of no satisfactory experiment to that effect (a perfectly correct answer, for up to the date of the

poisoning of Cook, strychnia had not been found in the blood and tissues), the counsel immediately suggested Orfila's experiment, the results of which had not the slightest bearing on the detection of absorbed strychnia. Mr. Grove knew at the time, or had the means of knowing, that Orfila's single experiment was performed in May, 1827, *i. e.* twenty-nine years ago, by mixing in a jar about five grains of acetate of strychnia, dissolved in a pint and a half of water, with some intestines (how much is not stated). The contents were exposed for three months, and then examined. In reference to this very experiment, Orfila so entirely distrusted the result which he obtained, that he makes use of the remarkable expressions elsewhere quoted (p. 90), "the phenomena of colour must not be relied on to demonstrate the existence of the poison:—strychnia, or its salts, must be so reproduced, that *all its characters* may be established."<sup>1</sup> Such are the facts on which Mr. Grove relied to induce the Court and jury to believe that *absorbed* strychnia had been found in the blood and tissues. Assuming that Orfila's experiment, of 1827, proved that unabsorbed strychnia

<sup>1</sup> *Nimum ne crede colori*, appears to be a rule in testing organic alkaloids, on which Orfila very strongly insists. On the value of the colour test for morphia—nitric acid, he observes, "This involves a serious question. Can a medical jurist rely upon *simple changes of colour alone*, as evidence of the presence of morphia, or its salts? Assuredly not, for it is not impossible that, under certain states of disease, the animal fluids may undergo unknown changes, and may give rise to one or more of these reactions. Such a conclusion should never be drawn, until morphia, or its salt, has been separated and obtained." ('*Toxicologie*,' ii, p. 231.)

In a case mentioned by him, MM. Ruspini and Cogrossi were completely deceived by the colour test for morphia,—iodic acid and starch, on which, on the authority of Liebig and Serullas, much reliance was formerly placed. They found that a decoction of the viscera of a calf, which had taken no poison, gave the same coloured reaction with the test as the supposed poisoned articles which they were engaged in examining. I have since heard of a case in which, from the application of the same test, morphia was supposed to be contained in and eliminated with the urine. It was subsequently found, that both lithic acid and the lithate of ammonia (constituents of healthy urine), produce the same change in iodic acid as morphia, and that this had given rise to the error. These remarks apply to strychnia and other alkaloids, as well as morphia. The colour-tests are useful, when we can obtain an alkaloid crystallized in substance; but the mere indications of colour, although they may give rise to suspicion, cannot be safely relied on as conclusive evidence. M. Devergie remarks ('*Méd. Legale*,' tom. iii, p. 17), that nothing is so deceitful as an absolute reliance upon colour in testing. Four persons may look at the same coloured product and it will be found to present to each a different shade or tint. This especially applies to those cases in which the quantities are extremely minute. (See Appendix.)

might be in contact with putrefied animal matter for a period of three months, without being entirely destroyed, it follows that, in order to apply it to the case of Mr. Cook, it must be supposed—1, that deceased had had a dose of *five grains* of strychnia; 2, that from the time of his taking the poison until his death, it had undergone no change in the living stomach; and 3, that the Crown witnesses had in these analyses as certainly to deal with the poisoned contents, as Orfila had with the contents of his jar.<sup>1</sup>

Devergie in the third edition of his work,<sup>2</sup> in his account of strychnia refers to Notus's test, namely, sulphocyanide of potassium; but, although, the "recognised colour tests" had been known for a period of nine years, he, like Orfila, does not even mention them. The whole of his third volume, is devoted to toxicology, and there is no fact recorded to show that strychnia has ever been detected in the stomach, the blood, or other tissues.

M. Galtier, the second edition of whose work was published in 1855,<sup>3</sup> enters minutely, in a volume on 'General Poisoning, into the processes for detecting the alkaloids, such as strychnia but he relates no instance in which strychnia has ever been detected in the dead body either in the unabsorbed or in the absorbed state. He condemns, as uncertain and unsatisfactory, any tests applied to liquids or extracts obtained from animal matter, and insists that in organic toxicology it is indispensable to obtain the alkaloids in a pure state, in order that their physical and chemical qualities may be determined.<sup>4</sup>

<sup>1</sup> There is only one experiment in Orfila's work which at all bears on the question. This is in reference to the *sulphate of brucia*. Orfila gave to a dog about five grains of this salt dissolved in two ounces of water. The animal was hanged in two hours. The liver was immediately removed, cut into pieces, and digested in boiling alcohol. After boiling for a quarter of an hour, the decoction was filtered, and evaporated to a soft extract. This was mixed with a little water, and from the action of nitric acid and chloride of tin, it was inferred that it contained brucia. As no attempt was made to get rid of the organic matter, this result cannot be considered satisfactory. Orfila himself objects to any inference in a case of poisoning being drawn from these colour tests so applied. To affirm its presence, brucia must be separated, as such, and its different chemical characters established. ('Toxicologie,' ii, p. 597-9.)

<sup>2</sup> 'Médecine Légale Théorique et Pratique,' par Alph. Devergie, Paris, 1852, troisième édition.

<sup>3</sup> 'Traité de Toxicologie,' 3 tomes, Paris, 1855. <sup>4</sup> Op. cit., tom. ii, p. 250; iii, p. 104.

M. Flandin, another French author, who has written a most elaborate treatise on poisons,<sup>1</sup> enters more into this subject than the preceding authors; but even here there is nothing satisfactory in relation to the question which we are considering. He has "poisoned animals in his laboratory (how many or what animals he does not state) with from three quarters to one grain and a half of strychnia," and he says, "I have constantly found this poison in the contents of the intestines (*matières intestinales*). Will it not equally be found in the liver and other organs into which it shall have been carried by absorption?" He thus answers his own question—"I have not had time to pursue these researches." (Tome ii, p. 255.) Thus instead of a fact we have a speculation. The tests on which he relied are not even stated, but the only one of the colour tests mentioned by him in his list of the chemical reagents for strychnia is sulphuric acid and peroxide of lead. M. Flandin has, therefore, done no more than the witnesses in Palmer's case. He has given comparatively large doses of this poison to small animals, and has then looked for it and found it in the unabsorbed state in the alimentary canal. In his two volumes, he reports no case of poisoning by strychnia in the human subject, in which that poison has ever been found in the dead body.

He quotes the process for the alkaloids recommended by M. Stas, a Belgian chemist, who, in 1847, undertook the chemical investigation relative to the poisoning of the Count Bocarmé by nicotina,—a process which is probably the best that has yet been suggested. "On three occasions," M. Stas observes, "during a period of six years, I have discovered the alkaloids in cases of poisoning: in 1845, at Bruges, morphia was detected in the viscera of a body after an interment of thirteen months; in 1847, morphia was also detected in the viscera of Bureau; and about this period, I detected aconitina in a suspected liquid which had become considerably changed (*profondement alteré*)." The quantity of morphia taken by the deceased persons, the period which they survived, the quantity found in the viscera, the viscera in which it was detected, and the tests upon which M. Stas relied to swear positively to the presence of morphia

<sup>1</sup> 'Traité des Poisons, ou Toxicologie appliquée à la Médecine Légale, à la Physiologie et à la Thérapeutique,' par Ch. Flandin, tom. i, ii, Paris, 1853.

and aconitina, are not mentioned. Although no cases are given, or facts mentioned, M. Stas sums up his memoir by the general statement—"I have applied the principles just laid down (by his method of research) to morphia, codeia, strychnia, brucia, veratria, emetina, colchicina, aconitina, atropia, and hyoscyamia; and I have been able, without the slightest difficulty, to separate these different alkaloids *when previously mixed with foreign matters.*" These results, however, cannot be taken as referring to the separation of the poisons above-mentioned from the viscera of human beings or animals which had taken them during life; for on this subject there is no account of a single experiment. The analysis refers to the separation "of strychnia and brucia from nux vomica, veratria from the extract of veratria, emetina from the extract of ipecacuanha, colchicum from the wine of colchicum, aconitina from an aqueous extract of monkshood, hyoscyamia from a very old extract of henbane, and finally atropia from an old tincture of belladonna."<sup>1</sup> Facts of this description have a pharmaceutical interest, but until the results have been verified by repeated trials in the dead body, they are of very little value to the medical jurist. Some of the poisons which he mentions will destroy life in a minute fractional proportion of a grain: and no process, however delicate, can make up for a small quantity of poison distributed by the circulation through an enormous mass of animal matter.

In reference to vague statements of this kind, I agree with M. Galtier, who says, "that authors in affirming that they have *detected* these poisons in the bodies of men and animals, in the matters ejected, or in the contents of the alimentary canal, have in general preserved a discreet silence on their processes for separating them, and on the nature of the tests by which they professed to recognise them."<sup>2</sup>

The most recent practical work on processes for the detection of poisons is that of Dr. Otto, Professor of Chemistry in the University of Brunswick, the discoverer of the colour-test for strychnia, (bichromate of potash and sulphuric acid) upon which so much has been lately said and written.<sup>3</sup> This writer,

<sup>1</sup> Flandin, op. cit., tom. ii, p. 143.

<sup>2</sup> Galtier, op. cit., tom. ii, p. 251.

<sup>3</sup> 'Anleitung zur Ausmittlung der Gifte,' von Dr. Fr. Jul. Otto, Braunschweig, 1856.

after speaking of the great difficulties attending the search for strychnia in organic liquids, dwells with minuteness on the method of detecting this alkaloid, and the application of his own colour-test, but he relates no case in which he has ever applied it or known it to be applied, for the detection of strychnia in the absorbed or unabsorbed state in the dead human body.

With these facts before us it becomes necessary to examine the grounds on which it has been alleged that strychnia can *always* be detected in the dead body. The first witness in support of this proposition was Mr. Herapath.

Mr. GROVE.—Q. Have you seen any case in which you have examined the human body where strychnia has been taken? A. Yes, in *one case*. I had the contents of the stomach. Q. Did you recognise it in that state on chemical tests being applied? A. I discovered it in the contents of the stomach. Q. How long after death? A. It must have been three days after death. Q. What was the test you used? A. I used common sulphuric acid and the bichromate of potash (the colour-tests). Q. Have you experimented on animals? A. I have. Q. To a large extent? A. Not to a large extent; sufficient to establish the principle. Q. Can you tell me to what extent? A. In my own experiments and those I have witnessed, eight, nine, or ten. \* \* \* Q. Confine your attention to those which you have analysed? A. There are *two* which I have destroyed myself; they were cats. I gave *one grain* of strychnia in a solid form. In this animal I found in the urine which had been ejected, strychnia; and I also found it in the stomach by the tests mentioned. \* \* \* The second case was also a cat. I gave the same quantity (*one grain*) in a solid form in food. \* \* \* Q. Did you find strychnia? A. Yes; in the urine, in the stomach, and upper intestines, in the liver, and in the blood of the heart. Q. In *all cases* in which you have seen a chemical examination or a search for strychnia, has it been found? Did you detect it by the same tests? A. Yes; but I went a step beyond—"I took extraordinary precautions to get rid of the organic matter." Q. In *all cases which you have seen*, when strychnia has been taken, has the examination been successful? A. Yes; not only strychnia, but nux vomica.



\* \* \* Q. Are you of opinion, as a chemist, that where strychnia has been taken in a sufficient dose to poison (to destroy life?) it *can be detected* and *ought to be detected* by chemical science? A. Yes, up to the time the body is decomposed completely. I mean even where there is putrefaction, &c.

*Cross-examination.*—When you have first experimented for the purpose of finding strychnia, has that been principally in the stomach? A. *Until lately.* Q. *When did you first look for it in the tissues of the body?* A. *On the 8th May.*<sup>1</sup> Q. With a view to this case? A. Certainly; it was with a view of learning everything that I could with respect to strychnia. I have experimented in about nine instances to find strychnia; they were not chemical experiments made by myself but things sent to me to analyse.

Mr. GROVE.—Q. What is the smallest quantity you have detected in the stomach? A. I can show you some which I extracted from a dog; *if* the strychnia is perfectly free (from organic matter) I am certain that I could discover the *fifty thousandth part of a grain.* By the ATTORNEY-GENERAL—Q. Suppose a grain had been administered to an animal, how much should you expect to find in the heart? A. Very little indeed; it is a difficult experiment, and requires great precaution, but it can and has been shown. Mr. GROVE.—Q. What proportion? A. I cannot tell; I have no idea, but it must be a very small quantity, supposing that there are twenty-five or twenty-six pounds of blood in the human body. I found strychnia in an ounce and a half of the blood of a dog weighing twenty-five pounds (poisoned by one grain). With the eighth part of the liver of this dog I made four distinct experiments with the four tests,<sup>2</sup> so that I experimented upon

<sup>1</sup> It is worthy of observation, that it was just prior to this date, that the paper elsewhere published (*ante*, p. 60) containing the details of the chemical processes adopted by Dr. Rees and myself, in Cook's case, was sent to the solicitor for the prisoner, for the special information of his chemical witnesses. From that document they would learn, that we had examined the coats of the stomach for strychnia, and *that the tissues of the body* had not been examined for this poison. Their experiments, which must have been somewhat hastily prosecuted, as the trial commenced on the 14th May, may have been for some time contemplated; if so, it is only a very remarkable coincidence.

<sup>2</sup> The four colour-tests, *i. e.*, sulphuric acid with bichromate of potash, peroxide of lead, peroxide of manganese, and ferricyanide of potassium.

the *thirty-second* part of the liver. I could see the violet colour in that minute portion—it changed to purple, passing to red afterwards.

Mr. RODGERS, the next witness, deposed that he had performed *one* experiment on a dog. He poisoned the animal in December last, with *two grains* of strychnia. He analysed some of the blood when putrid, ten days after the death of the dog (how much blood is not stated). He separated the strychnia, or rather determined its presence, by the colour tests. He *had never analysed any portion of the human subject for strychnia.*

Dr. LETHEBY.—I have witnessed many cases of death by poisoning from strychnia—many of the lower animals—and several cases of poisoning by nux vomica in the human subject, one of which was fatal. \* \* \* I do not hesitate to say, *of all poisons, either mineral or vegetable, strychnia is the most easy of detection after death.*<sup>1</sup> I have detected it in the *stomachs of animals*, in numerous instances. I have detected it in *the blood and in the tissues.* (The date when, the quantity of matter examined, the kind of animal, and the doses given to the animals are not stated.) \* \* \* I have succeeded in detecting very minute portions of strychnia. *When it is pure* it can be detected in very small fractions of a grain, at least the *twenty thousandth part of a grain*; when mixed up with organic matter it would be more difficult of detection, no doubt; still, I can detect the *tenth part of a grain in a pint of any liquid* that it may be put into. \* \* \* I have detected it in a pint of the most putrid liquid that I could obtain, in which the tenth part of a grain had been standing for two months.

Mr. KENEALEY.—Q. You have told us you have succeeded in detecting it in an animal which had been killed a month, and was in a state of decomposition; what was the dose you

<sup>1</sup> Dr. Letheby is certainly not of the opinion of Dr. Otto, the discoverer of the very delicate mode of colour testing, on which he (the witness) relies. Writing in 1856, this excellent chemist says, "The separation of very small quantities of the alkaloids (strychnia and morphia) from articles of food, contents of the stomach, &c. requires the greatest experience, and a most practised hand, and yet this separation in the purest condition possible, is necessary, because a very minute intermixture of any foreign substance renders the tests used for their detection, fallacious." ('Anleitung,' p. 88.)

had given? *A.* I gave the animal (a rabbit) half a grain, and I have the *strychnia* here within a fraction of what I gave him. LORD CAMPBELL.—*Q.* In what proportion? *A.* I lost about the tenth part of a grain in the course of the investigation.<sup>1</sup>

The evidence of Dr. Wrightson includes the following points. He stated that he had found *strychnia* in the blood of a dog poisoned by two grains, in the urine of another dog poisoned by one grain, and in the viscera of a cat poisoned by half a grain. He stated, in answer to a question, "If a man had certainly been poisoned by *strychnia* he should certainly expect to discover it. *Not generally, but certainly.*"

Mr. Nunneley stated in his evidence that he had experimented with *strychnia* in sixty animals of various kinds. In the case now known as that of Mrs. Dove, he had been engaged with Mr. Morley, and they had detected, by the colour-tests, *strychnia* in the contents of the stomach. In this case, however, after the chemical experiments had been performed, four animals were killed with the residue of the contents. He had himself never detected *strychnia* in the blood or tissues. *He never tried to extract it.* He saw the experiment performed by Mr. Herapath, and the *first time* he had seen it done was *only the day before the trial.*

*Question* by Mr. SERGEANT SHEE.—Supposing death to have been caused by a dose of *strychnia*-poison, sufficient but not more than sufficient to destroy the animal, in your judgment would it be so decomposed by the process of absorption

<sup>1</sup> It is difficult to make any satisfactory deduction from this evidence. It is not stated how the *strychnia* was given to the animal, nor from what parts of the body it was subsequently extracted. If *strychnia* be given wrapped in paper, the greater part may be recovered from the stomach after death (see experiments, *ante*, by Mr. Devonshire and Mr. Horsley, p. 68, also p. 120). If so given, that the greater part is absorbed and removed from the stomach, then it follows that *all* the parts of the body to which the *strychnia* is carried by the blood, must be made to yield the poison, or so large a proportion could not be recovered. Mr. Herapath found a portion in the urine, and Mr. Rodgers, the preceding witness, states that he has discovered it in the bones. Hence, it follows, that all the excretions, as well as the skeleton must be analysed, before the whole of the *strychnia* administered can be recovered within a very small fraction of a grain. The result as it stands appears to show that a rabbit may be killed by the tenth of a grain, or if this were merely lost in the investigation, then it follows that the *strychnia* must have killed the animal by some catalytic property, and after penetrating his whole body, have come out as little changed as silica or spongy platina!

that you would not be able to detect it by these tests (the colour-tests), in any portion of the system? *A.* No. LORD CAMPBELL.—That would be what is called *a minimum dose*, enough and no more.

In answer to a question put by the Attorney-General, this witness stated that the dose which he had generally given in his experiments was “from *half a grain to two grains.*” The animals experimented on were “dogs, cats, rats, mice, guinea-pigs, rabbits, frogs, and toads.”<sup>1</sup>

<sup>1</sup> This witness it will be perceived has had a better opportunity of solving the question now at issue, on the detection of absorbed strychnia, than any one of the chemical witnesses who appeared for the defence. The case of Mrs. Dove occurred in March, 1856. This lady, it is well known, had been poisoned by repeated doses of strychnia, given over a period of a week. She died about twenty minutes after the last dose, which must have been a large one, as the contents of the stomach, after sufficing for numerous experiments and the application of the colour-tests, still contained enough strychnia to poison *four animals*. Mr. Nunneley had taken an interest in Cook’s case, and knew that strychnia had not been detected by Dr. Rees and myself. He admitted to the Attorney-General, that he first communicated with the solicitor for the prisoner, Mr. Smith, by sending to him a report of Dove’s case. The finding of strychnia in Mrs. Dove’s stomach was thenceforth made the great point on which it was alleged that Cook could not have died from this poison. The importance of examining the tissues in the case of Mrs. Dove was therefore obvious, and no case could have been more favorable than this for such a scientific research. Mr. Nunneley had access to the blood, and the whole of the viscera in the case of this lady. He had the most perfect confidence in his processes and tests for this poison, for in one experiment on the stomach of an animal he believed the poison to be present, when Mr. Morley, his coadjutor, doubted the result. He is asked by the Attorney-General: *Q.* Did you know that this poison could be extracted by chemical processes from the tissues? *A.* I thought it *probable*. *Q.* You never tried it? *A.* No. Why, if he thought it “probable,” did he never try the experiment upon the blood and tissues of any one of the sixty animals, poisoned by him with from half a grain to two grains of strychnia, or upon the blood and tissues of the lady, the whole of whose body was placed at his disposal for analysis, at the very time when the country and profession were calling for facts in reference to the detection of strychnia! Instead of resorting to this proceeding, he witnesses an experiment tried by another person only the day before the trial of Palmer, and then, without any direct experience whatever on the subject, swears, in the most positive manner, that no person can die from a minimum dose of strychnia without the poison being detected by those tests (the colour-tests), *in any portion of the system*. This witness, it appeared, voluntarily gave the benefit of his advice and experience to the solicitor for the defence. Supposing the blood and tissues of Mrs. Dove to have been analysed, and strychnia had *not* been found, or that he and Mr. Morley had differed as in a former experiment about the results, the reader will perceive that all the evidence respecting the extraction of strychnia from the tissues of cats and dogs

It appears to me, it must be obvious from this report and analysis of the evidence of the medical witnesses for the defence, that they have adduced no satisfactory reasons for the very strong opinions which they gave on the infallible detection of strychnia in all cases of poisoning by it. The only witnesses who had had an opportunity of making an analysis in the human subject were Mr. Herapath and Mr. Nunneley. In cases presenting no difficulty whatever, they found the poison in the contents of the unmutilated stomach. That strychnia may be detected in this organ in certain cases all the witnesses are agreed; but those for the prosecution think that such circumstances as a minimum fatal dose, absorption during an hour or two that the patient may survive, and the tampering with the stomach and losing its contents, wholly or in part, may lead to an occasional failure of the chemical processes: those for the defence think otherwise. This is the essential point of difference so far as the *stomach* is concerned. Time will probably show which is the more correct view.

As to the *blood and tissues*, not one of the witnesses for the defence had ever made an examination of these parts in the human body for *absorbed strychnia*. Two of them, Mr. Herapath and Mr. Nunneley, had each had an excellent opportunity of determining this question practically in the cases which respectively fell under their notice; but for some reason or other neither took advantage of it.<sup>1</sup> The unabsorbed

hastily made a week before the trial, would have gone for nothing. The opinion which Dr. Rees and I had entertained, in the case of Cook, that strychnia cannot with certainty be found under such circumstances, would have been corroborated, the time of the Court saved, and a great scientific question practically solved.

<sup>1</sup> It is well known that *nux vomica* owes its poisonous properties to the strychnia absorbed from it. Mr. Herapath stated in his evidence, that he had found *nux vomica* in a fox and two dogs poisoned by it. Dr. Letheby had also met with one fatal case of poisoning by *nux vomica* in the human subject. How did it happen, that these gentlemen only sought for *nux vomica* in the stomach? *Why not have searched for strychnia in the tissues?* As a matter of science, this was of great importance; but the problem on which so much speculation has arisen was left unsolved when there were favorable opportunities for its solution. I need hardly remark, that the accuracy of these hyper-chemical views would be severely tested by a research of this kind. One hundred grains of *nux vomica* contain only about half a grain of strychnia, mixed with *brucia*, therefore less than half a grain of the poison the properties of which we are now considering. Thirty grains of *nux vomica* (= to one sixth of a grain of strychnia) have proved fatal to an adult. Can

strychnia remaining in the stomach was all that these gentlemen looked for, while at the same time they have both shown an amount of confidence in the processes for detecting it in the parts of the body in which they had never sought for it that is perfectly astonishing. The research for absorbed strychnia in animals was, so far as the evidence goes, not made by any one of these witnesses until *after* the question had arisen in reference to Mr. Cook's body. The first discoveries of the absorbed poison were made by Mr. Herapath on the 8th of May, the trial on which the evidence was to be given commencing on the 14th! He found strychnia in the blood and liver of one dog, in the urine of one cat, and in the urine, liver, and blood of the heart of another cat. Each of these animals had been poisoned by a dose of *one grain* of strychnia.<sup>1</sup> Mr. Rodgers's experience is confined to the detection of strychnia in the blood of one dog, to which he had given *two grains* of the poison. Dr. Letheby adds to this evidence, that he has detected it in the blood and tissues of animals, the doses given not being stated. It does not appear that he has ever had an opportunity of searching for absorbed strychnia in a human being who had been poisoned by it. Dr. Wrightson detected

the strychnia from a small and fatal dose of nux vomica be detected in the blood, liver, and urine? Nothing would be more easy than the performance of Dr. Christison's experiment of injecting the *sixth part* of a grain of strychnia, in solution, into the cellular membrane of a dog. The animal, in his experiment, died in two minutes. There is nothing to prevent the chemist testing the accuracy of his processes, by seeking for absorbed strychnia in such a case in the blood, liver, and tissues generally. Strychnia given in large doses by the stomach, may impregnate the liver and other adjacent organs by exosmosis and thus lead the chemist into the fallacy of supposing that he had discovered "absorbed" strychnia. At any rate, before very strong inferences are drawn from the presence of a minute quantity of strychnia in the "thirty-second part of the liver" of an animal, it is only proper that all possible sources of error should be excluded. Dr. Kidd found that the liver of a dead animal might be impregnated with arsenic by imbibition from the liquids of the stomach. Matteucci long since proved that, if the hind legs of a frog recently killed were immersed for some hours in a solution of ferrocyanide of potassium, every part of the viscera was so penetrated with the salt by imbibition, that by touching them with a solution of a persalt of iron, a blue stain more or less deep was produced.

<sup>1</sup> This, it will be observed, is comparatively a very large dose to give to such animals. It is equal to, at least, five or six grains given to a human being. The Court was desirous of information on the process for detecting strychnia where the dose was at a minimum, and this was the kind of evidence supplied!

it in the blood of one dog poisoned by two grains, and in the urine of another poisoned by one grain; also in the viscera of a cat (absorbed?) destroyed by half a grain. Of Mr. Nunneley's experiments I have already spoken; he has never detected it in the blood or tissues. Even in reference to the bodies of the four animals poisoned by the contents of the stomach of Mrs. Dove, he did not seek for the poison (in the blood or tissues) in any one instance, although this would have thrown great light on the power of chemistry to detect absorbed strychnia in instances in which it was known to have destroyed life.

The whole of the chemical defence resolves itself into this. From experiments made since the death of Cook, on *four dogs* and *two cats* ("a large and tried experience," according to Mr. Sergeant Shee), without a single fact derived from the examination of the human body, the witnesses for the defence considered themselves justified in asserting that no *human being* could die from the effects of a minimum dose of strychnia without the poison being detected.<sup>1</sup> The question then is: Are these facts, even if they were multiplied tenfold, sufficient to warrant the assertion that strychnia, when operating in *minimum* doses, cannot possibly escape detection in the stomach or in some portion of the system? This question will receive a solution according to the knowledge and experience of those who are engaged in such researches. We have already seen from the record of cases that the poison has not hitherto been found in the tissues of persons poisoned by it; and further, no toxicologist has recorded an instance in which he or others have succeeded in detecting it.

The case of Dr. Warner (No. 9 in the table, p. 81), who died from the effects of half a grain of sulphate of strychnia, corresponding to 0.43, or forty-three hundredths of a grain of pure strychnia, will enable us to test the accuracy of the opinion thus strongly expressed. This being a soluble form of strychnia, if we assume that the whole was removed from the stomach by absorption in an hour, the proportion of strychnia would not amount to so much as one fiftieth of a grain in a pound of blood, or less than one eight-hundredth of a grain in an

<sup>1</sup> Dr. Letheby did not state in what number of animals he found strychnia in the blood and tissues.

ounce.<sup>1</sup> If, in the short space of twenty minutes, during which the deceased survived, one half only was removed by the absorbents, then one quarter of a grain might be detected in the stomach; but in the blood the proportion would be no more than one hundredth of a grain in a pound, and less than this proportion in any of the tissues. In the experiments on which the chemists for the defence relied in order to support their theory, they gave to the animals, weighing from one seventh to one tenth of an adult human being, doses of strychnia twice and four times as great as the quantity which had actually proved fatal to Dr. Warner!

Dr. Geoghegan, Professor of Medical Jurisprudence at the Royal College of Surgeons in Dublin, observes, in commenting on the medical evidence in this case,—“With the views which would claim for the chemical evidence a positive result in *all* cases of strychnia-poisoning, I can in no wise concur. Nor does the testimony given at the late trial on this head appear by any means sufficient to establish a proposition so striking, and yet so little in accordance with what might, *a priori*, have been expected on physiological grounds. Thus, in the case of the experiments on the part of the defence, the dose administered was always large in proportion to the size of the animal. Hence the only legitimate inference appears to be, that where a similar proportion obtains in the human subject, and under accompanying conditions also alike, the poison should be discovered; and accordingly it was so in the two cases above alluded to, both of them instances of suicide, where usually much more is taken than is sufficient to cause death. In charges of murder by poison, on the contrary, the investigation is not uncommonly conducted under circumstances of a very different kind; *for should the administrator possess a certain degree of skill, the dose may be so apportioned that the detection*

<sup>1</sup> The quantity of blood in the body of an adult is here estimated at twenty-five pounds. Haller considered that it amounted to about one fifth the weight of the body, and thus it would be twenty-eight pounds in a person weighing one hundred and forty pounds. Todd and Bowman give the results of Valentin, who assigns thirty-two pounds for a man between thirty and forty years of age, and twenty-eight pounds for the female; but they say there is no reason to infer that the quantity exceeds thirty pounds. These statements show that the quantity on which the calculation is based in the text, is taken at a low average.



may become either difficult or impossible. A case is on record in which *half a grain* of sulphate of strychnia (less than the quantity of the uncombined alkaloid) caused speedy death; and, on the trial of Palmer, another was given in evidence, where the same result ensued from *three quarters of a grain*. Now it must be obvious to any who have practical experience in these matters, that under such circumstances the probability of its detection would be comparatively small, whilst, regard being had to the vastly greater mass of the blood and tissues in which it would in such cases be distributed, as contrasted with those of animals (the dog or cat) made the subject of express experiment, the chance of its unexceptionable discovery in the latter quarters might be equally slight."<sup>1</sup>

In these remarks we have the opinion of an independent man, of skill and experience, whom those engaged for the defence of Palmer were most desirous of bringing forward as a witness. Feeling, however, from what he knew of the case, that death from the action of strychnia was clearly substantiated, he declined the invitation to attend.

The opinions of the medical witnesses for the prosecution were very much in accordance with the view taken by Dr. Geoghegan. Dr. Christison stated that he should not expect to find strychnia in the body after death where the quantity taken was small. If the excess left in the stomach over that removed by absorption were considerable, he would expect to find it. Mr. Morley said, where small or minimum doses were given, he should sometimes expect to fail in its detection. Hence it follows, if a minimum fatal dose could not always be detected in the stomach, it would be still less likely to be found when diffused through the whole mass of the body by absorption.

But what meaning is attached to the word "detection?" One half of all scientific controversies are properly referred to some misunderstanding of the meaning of the words employed. From the statement made by Mr. Herapath, that he could "detect" 1-50,000th of a grain of strychnia; and by Dr. Letheby, that he could "detect" the tenth part of a grain in a pint of any liquid that it may be put into, *i. e.*, when diffused through 87,500 times its weight of organic matter; it is quite clear that the witnesses for the defence rely upon the production of a transient purple colour,

<sup>1</sup> On Poisoning by Strychnia, from 'Dublin Medical Press,' 1856, p. 9.

produced by adding to this infinitesimal quantity of strychnia a mixture of sulphuric acid and bichromate of potash, or some chemical agents that will produce similar effects. There can be no doubt that this is a very "delicate" process: but, in questions of murder, a Court of law requires certainty rather than delicacy. Marsh's process for arsenic is very delicate: it will give a metallic deposit when the millionth of a grain is present; and Reinsch's process will give a coloured deposit on copper when the arsenic is in a proportion almost as small. At first, chemists were inclined to rely upon the visible appearance of such deposits alone, as positive evidence of the presence of arsenic; and it is well known that such a reliance led even the experienced Orfila into the belief that arsenic was a normal constituent of every human body. This was proved subsequently to be an error by a Commission appointed to examine a scientific question involving such serious consequences. Chemists are now universally agreed not to rely upon the results obtained by these "delicate" processes for arsenic, except when the quantity of deposit is such as to admit of the application of some correcting test or process. Are we to demand less of Otto's colour-test for strychnia than of the processes of Marsh or Reinsch for arsenic? In operating on minute quantities of thousandths of grains, it is clear that there can be no corroboration; the judgment must be based on the change of colour produced: and here wide differences of opinion may arise. A very remarkable example of this occurs in the evidence given by Mr. Morley and Mr. Nunneley, the two gentlemen who conducted the investigation in the Leeds case, and who appeared, the one as a witness for the prosecution, and the other for the defence, at the trial of Palmer. In reference to the detection of strychnia in all cases in which it had been given to animals, the following question is put to Mr. Morley:

Mr. GROVE.—Q. I think you said in one or two you speak doubtfully? A. In one certainly; in that case we were sure that strychnia had been administered, our doubt was whether it had reached the stomach; in *that* case I may say there were appearances which a *sanguine eye* might say were those of strychnia—a *more cautious one would doubt*.

The chemical evidence given for the defence would lead to the inference that the colour-test is all in all sufficient

for the detection of strychnia, and that it is exposed to no sort of fallacy. But it is obvious that the relative shades of colour produced will receive different interpretations according to the more or less sanguine views of those who are engaged in the analysis. The question, therefore, for a Court of law is—Is this a satisfactory ground on which to receive a positive opinion of the presence of strychnia on a charge of murder, when the opinion may be the turning point of the prisoner's conviction or acquittal? The practical utility of the colour-test applied to the tissues, and the amount of courage possessed by a witness in relying upon it, cannot be fairly tried until a case of this kind has presented itself.

Certain organic compounds, such as pyroxanthine, salicine, and aniline,<sup>1</sup> produce in very minute proportions, colours, on contact with sulphuric acid, which might, in some instances, be confounded with the effects of small quantities of strychnia, where an eye was very eagerly engaged in looking for this poison. A drop of cod-liver oil produces with sulphuric acid, with or without bichromate of potash, a play of colours that I have known to be mistaken for the action of strychnia. There is no difficulty in distinguishing any of these substances from each other chemically, but such reactions with organic matter should, it appears to me, teach caution in drawing inferences. Has the liver, tissues, or blood of a person who has been long under a course of cod-liver oil been examined to determine how far a fallacy might arise by over-confidence in the production of colour when sulphuric acid and bichromate of potash are added to the extracts derived from these parts? It is perfectly true that some of the substances mentioned are not likely to be found in the stomach, and that sulphuric acid produces a colouring effect upon some of them without the addition of bichromate of potash. In reference to the first point, the test has only been known nine or ten years, and it has been so little tried on the fluids of the dead human body, that we can hardly affirm, where the colouring effects are slight, that some combinations of animal matter might not create deception; in reference to the latter point the result will depend on the mode of applying the test. Thus, Schneider, one of the most recent writers on poisons, advises that the supposed strychnia should be

<sup>1</sup> See Appendix, page 151.

first rubbed with peroxide of manganese or other material used, and sulphuric acid then poured on the mixture.<sup>1</sup> There would be no escape from a fallacy in following such advice; and it is astonishing how easily the eye is deceived when anxiously looking for an anticipated result. The order in which the substances are used in the application of the colour-test is then most important; but the real danger of a fallacy is precisely in those cases in which the quantity of material is so minute that there is not enough for any corroborative test, either by taste or crystallization.<sup>2</sup>

*Experiments on animals.*—At the trial, evidence was given for the prosecution in reference to the detection of strychnia in the bodies of animals poisoned by this substance. It may be proper to place these facts on record, as well as the results of other experiments since made by Dr. Christison and Dr. Maclagan, and by myself, with an anxious desire to arrive at the truth. The animals employed were rabbits, healthy and full grown. The first set of experiments were performed in December, 1855.

EXPERIMENT 1.—*One grain* of sulphate of strychnia, mixed with conserve of roses, was given to the rabbit in the form of a pill, which it readily swallowed. The animal appeared very well for *seven minutes*, when it fell upon its side, and was suddenly seized with a trembling of the whole of the body and limbs, followed by a violent convulsive fit, attended with opis-

<sup>1</sup> 'Die Gerichtliche Chemie,' Wien, 1852, p. 316.

<sup>2</sup> One of the witnesses for the defence, Mr. Rodgers, professes, since the trial, that he has extracted strychnia from the *bones* of animals poisoned by it, and that in cases of suspicion there may be an expectation of finding it in any undecayed parts of skeletons. Is it really strychnia that has been thus extracted, or is it the *ignis fatuus* of the colour-tests which is misleading this experimentalist? The careful experiments of Millon and Laveran show that in mineral poisoning it requires a period of *several months* for the metal (antimony) to *enter the bones*. Mr. Rodgers is dealing with a poison which kills by minutes, and yet the osseous structure is supposed within this brief period to become so impregnated with the poison, as to yield strychnia on chemical analysis! An experienced chemist informs me that he has tried Mr. Rodgers's process by hydrochloric acid, and it did not succeed. Dr. Stevenson Macadam found that this process caused a failure in his experiments. At present the statement appears to me, so far as the colour-tests are concerned, to be a *reductio ad absurdum*. I shall be induced to modify or withdraw this opinion, when this gentleman has poisoned an animal with a dose of crystallized strychnia extracted from the bones of a poisoned animal.

thotonos. The fore and hind legs were rigidly extended, and the body hard and stiff. When forcibly straightened, the head was again drawn back, and the spine assumed the form of a bow. There were repeated jerking motions of the legs, followed by short intervals of relaxation. The spasms were brought on by the slightest causes. Just preceding a convulsive fit, the animal uttered a cry, which was several times repeated; this was indicative of pain. The eyes were wide open and protruded, the pupils dilated; the breathing was difficult, and entirely abdominal. After a succession of fits, at shorter and shorter intervals, the animal died in a violent convulsion, in which the body was drawn into a bow, and the limbs were perfectly fixed. Its death took place, *i. e.*, its heart ceased to beat, six minutes after the commencement of the spasms, and *thirteen* minutes after swallowing the poison. When examined, after a few minutes, the body was found perfectly rigid, in the attitude in which the animal died. This rigidity lasted several days. The cavities of the heart were distended with blood partially coagulated. The lungs were congested, the liver natural. The stomach and intestines presented an opaque-white appearance. The blood-vessels were distended with dark-coloured liquid blood. There was no remarkable fulness of the vessels of the brain or its membranes. The upper part of the spinal marrow and its investing membranes were quite healthy.

The stomach was secured by ligatures, and, with its contents, removed for analysis, as well as the liver, lungs, and heart, with the blood contained in the last-mentioned organ.

EXPERIMENT 2.—A pill containing *one grain* of Morson's crystallized strychnia was given to this animal. Although the strychnia appeared to be of good quality and was found to contain but very little brucia, no symptoms were observed for the long period of an hour and five minutes. A pill containing a grain of another sample of strychnia (making *two grains* in the whole) was then given to it. In *nine minutes* after this second dose the animal was suddenly seized with a convulsive fit, by which its body was jerked violently off the table on which it had been placed. There was the same general tremor over the body and limbs as in the preceding case, appearing like a rip-

ple passing over the surface of water. There was opisthotonos; the fore legs were quite rigid and stretched in a straight line, a little inclined backwards, with the most extreme tension; the intercostal muscles were fixed, so that the breathing was entirely abdominal. The eyes were fixed and prominent, a condition especially observed at the time of the attack of convulsions. There was at this time a frequent cry, as from great pain. When the body was straightened, the tail and head were speedily drawn back, and the spine arched, in the most complete state of opisthotonos. The heart pulsated rapidly; during the fits its beats were 140 in a minute, and in the slight intervals of relaxation, 108. At times its action was hardly perceptible; but on the recurrence of a fit, its beats were so frequent that it was difficult to count them. There was a flickering motion of the muscles of the lower jaw and face. After a succession of fits, the heart suddenly ceased beating, and the animal died, thirteen minutes from the time of the commencement of the symptoms, and *twenty-two minutes* after the second dose of poison had been given to it. The body of the animal became rapidly rigid in the position in which it died; and in twenty-five minutes, while still warm, it was so rigid that it could be held out by the head and chest, or by the haunches, in a straight line, without collapsing. It retained this rigid condition for several days.

The heart was full of blood on both sides. The right lung was emphysematous (distended with air), the left lung natural. The liver was healthy. The blood was dark coloured and in a partially coagulated state. The stomach was very pale on the exterior. The blood-vessels were distended with dark-coloured blood. The intestines were healthy; the spleen was small and collapsed. The brain and spinal marrow were healthy; the membranes were somewhat congested.

The parts removed for analysis were the same as in the preceding case. In addition, a small quantity of blood was collected from the heart and vena cava for special examination.

EXPERIMENT 3.—*One grain* of strychnia was given to this animal in a pill. The symptoms appeared suddenly in *nine* minutes; they ran through their course in eight minutes, and the heart of the animal had entirely ceased to beat in *seventeen*

minutes. There was the same order as well as intensity of symptoms as in the preceding cases—opisthotonos, occasional cries of pain, and violent tremors of all the voluntary muscles of body and limbs, just preceding the convulsive fit. The pulsations of the heart during the fit reached 160. The body became stiff and rigid while warm, and remained as stiff as a board during many days. The jaws were fixed. Before death there were twitching motions about the muscles of the face.

The appearances in the chest and abdomen were similar to those met with in the other cases. The brain and spinal marrow were natural. There was no congestion of the membranes.

EXPERIMENT 4.—This animal took, in the form of a pill, *half a grain* of pure sulphate of strychnia. It had been twenty-four hours without food. The symptoms were similar to those already described; they appeared suddenly in *ten* minutes; in eight minutes from their commencement, and in *eighteen minutes* from the time of taking the dose, the animal was quite dead. The appearances in the chest and abdomen were the same as in Experiment 3. The brain and spinal marrow were in a healthy condition.

EXPERIMENT 5.—*Half a grain* of strychnia was given. The animal moved about, without appearing to suffer, for ten minutes. At this time it appeared to have an unsteady motion on its legs. Every attempt to move was attended with slight involuntary movements of the fore and hind legs. When moved, these irregular motions were well marked, and the animal appeared to cling by its feet with an unsteady hold to the table. At *twelve minutes* after taking the poison it was seized with a convulsive fit, and fell on its side in a state of opisthotonos. There was an interval of relaxation; but slight motions made near the animal, or touching it very gently, brought on the spasms with violence. The whole of its skin appeared highly sensitive. In a few minutes, and after several struggles with its hind legs, which did not appear to be under the control of the will, the animal was enabled to raise itself and walk about. It appeared as if it would recover. By touching and moving it, other convulsive fits supervened;

and in one of the most violent of these it died, eleven minutes after the commencement of the symptoms, and *twenty-three* minutes after the administration of the poison. Particular attention was given to the state of this rabbit at the time of death, and it was observed to be quite flaccid in its body and limbs. This condition lasted but for a short time. The animal was placed on its back, and the legs raised for the purpose of making an examination of the chest. In about ten minutes it became, while still warm, perfectly rigid in this raised attitude, the fore legs remaining stretched upwards, and wide apart. The rigidity in this animal had considerably decreased in two days, and had nearly altogether disappeared in four days.

The heart and lungs were full of blood. The stomach presented the remarkable paleness observed in the other cases. The brain and spinal marrow were healthy.

*Analysis.*—In Experiments 1, 2, 3, the stomachs were found full of green food in a partially digested state. In Experiment 4 there was but a small quantity of food. The stomach and contents in each case were digested in distilled water, acidulated with a sufficient quantity of diluted sulphuric acid to dissolve strychnia at a moderate temperature. The decoction was filtered, and the residue washed with warm distilled water until all soluble matters were removed. By evaporation, a clear yellow-coloured acid decoction was obtained, amounting to about six drachms. The liquid of No. 1 had a decidedly bitter taste; this was well marked in No. 2; but in Nos. 3 and 4 it was not perceptible to Dr. Rees or myself, although a friend imagined that he perceived it in No. 3. Carbonate of potash gave no precipitate in the decoctions, even after they had been allowed to stand for many hours. The extracts obtained from the evaporation of these decoctions were digested in alcohol, and the bitterness was perceived in the spirituous, as in the watery, solutions of Nos. 1 and 2 only. The process already described (*ante*, p. 60) was then pursued, as in the examination of the stomach of Cook. Faint, whitish, uncrystalline deposits were obtained by evaporation on glass, which, when treated with sulphuric acid and bichromate of potash, gave the following results: In Nos. 1, 3, and 4, no



change of colour upon which any reliance could be placed,—the bichromate was slowly converted to green oxide of chrome. In No. 2 the broken crystal of bichromate soon became surrounded with a rich violet-coloured liquid, passing rapidly through a purple to a light red colour. In the whole of these cases the hearts, lungs, and livers of the animals were separately examined for strychnia, and in No. 2 the blood collected from the heart and vena cava was also examined. The aqueous and alcoholic extracts had no bitter taste, and yielded no indication of strychnia by the application of the colour-test.

The process pursued in the analysis of the stomach was, for obvious reasons, the same as that adopted in Cook's case, but in reference to the *tissues*, we also employed animal charcoal for the purpose of separating the strychnia, according to the plan of Graham and Hofmann. The total absence of bitterness in any one of the extracts of the blood and tissues showed that there could be no appreciable quantity of strychnia. This, according to Dr. Christison's recent trials, is perceptible in cases in which the colour-test fails.<sup>1</sup> It is a property which cannot be concealed by organic matter, and to remove every objection on this ground, the one hundredth of a grain of strychnia was used as a counter-test. When mixed with the residue of each dial glass, in which the colour-test had failed to act, the colours were immediately produced in a clear and decided manner.

Although the tissues of No. 1 gave no indication of strychnia, the acid aqueous extract of the liver, heart, and lungs acquired by concentration, and without the addition of bichromate of potash, a rose-pink colour. It resembled the appearance presented by solutions of strychnia or veratria in sulphuric acid after exposure to heat, but there was no perceptible bitterness. It did not exist in the cases of the other rabbits, although treated in a precisely similar manner, and it may suggest a reflection as to the occasional production of colour in an extract from a dead body from the use of chemical reagents alone.

No analysis was made of the rabbit in Experiment 5. The body of this animal was opened, and the viscera, after exa-

<sup>1</sup> He has found no indication by the colour-test beyond the 15,000th grain of pure strychnia in *distilled* water, while he perceived the sense of bitterness in *distilled* water when the strychnia formed only the 40,000th of a grain. Taste is therefore in his opinion the best "guide test."

mination, were left exposed to undergo putrefaction. There was no perceptible difference observed in the time of commencement, or in the progress of putrefaction. It was noticed, however, that hundreds of larvæ of the musca carnaria and other insects were living and thriving in the midst of all the viscera of this animal. Strychnia, if present, certainly did not affect them.

Subsequently to the trial of Palmer I performed an experiment with a still smaller dose of strychnia than any which had been given in the preceding experiments, or by the chemists for the defence.

EXPERIMENT 6.—On the 3d of June, 1856, I gave to a full-grown healthy rabbit, which had been recently fed, *one quarter of a grain* of pure *sulphate of strychnia* dissolved in a few drops of distilled water. The animal swallowed the whole, excepting a small portion which adhered to the glass. It remained active for about *fifteen minutes*; at that time it appeared to be more easily startled by slight noises; its movements were tremulous, and it seemed to be unsteady in moving its hind legs. Soon afterwards it trembled violently, or started when touched. Slight twitchings appeared in the limbs at intervals on its attempting to move, or on making a noise. Eighteen minutes had now elapsed, and it had not been seized with a well-marked convulsion of the trunk. It was gently lifted from the table by the ears to be placed on the floor, when it was suddenly seized with a most violent paroxysm of convulsions. The hind and fore legs were rigidly stretched out, and there was complete opisthotonos. Its eyes protruded, its breathing was difficult; the pulsations of the heart could not be counted; the head and tail were drawn backwards as if by a tightened bowstring, with occasional slight intervals of relaxation, and in this state it died, two minutes after the commencement of the convulsions, and *twenty minutes* after taking the poison. Immediately after death, it was noticed that the whole body was flaccid, and there was a general relaxation of all the voluntary muscles; but the body speedily stiffened, and, what appeared remarkable, the fore limbs altered their position and became rigidly stretched out. In eight minutes from the time of death, while the body was still

warm, this rigidity of the muscles was perceived over the greater part of the trunk. On the 5th of June, the back and hind and fore legs were still rigid. On the 7th, the weather having been in the mean time very warm, the rigidity had in great part ceased; it was then only perceptible in the joints of the hind and fore legs.

At this date the body was examined. The stomach was pale, but much distended with food (probably amounting to half a pound of hay and green food). The coats were softened at the greater end. The intestines contained chiefly air; the lungs were collapsed, and of a bright red colour; the heart on both sides contained blood, partly coagulated; the blood in all other parts liquid and dark coloured. The parts of this animal taken for separate analysis were—1. The stomach and contents. 2. The blood of both cavæ, as well as that obtained from the right cavities of the heart, amounting with the coagula to two teaspoonfuls. 3. The lungs, the heart emptied of blood, the liver, and two kidneys; these organs being placed together for one experiment. 4. The whole of the muscles of the hinder extremities. The process pursued was that recommended by M. Stas.<sup>1</sup> Instead of using tartaric acid, however, I employed acetic acid, having found, by preliminary experiment, that this acid acted well on small quantities of strychnia. The concentrated acetic liquid obtained from the aqueous and alcoholic extracts of the different parts examined was rendered alkaline by potash, and then shaken with ether; the ethereal liquid was poured off, evaporated, and the residue tested. 1. From the stomach and contents whitish deposits of a semi-crystalline character were obtained. Sulphuric acid and bichromate of potash, as well as peroxide of manganese and the ferricyanide of potassium, gave, with these deposits, the violet, purple, and red colours indicative of strychnia. The alcoholic and aqueous extracts had a bitter taste. The quantity of strychnia obtained from the bulky contents of the stomach was estimated at not more than the one hundredth part of a grain. 2. The blood, carefully examined by the same process, gave not the slightest indication of strychnia, either

<sup>1</sup> This process, first suggested by the Belgian chemist for alkaloidal poisons generally, will be found described in Flandin's '*Traité des Poisons*,' tome ii, p. 137; also '*Edinburgh Monthly Journal*,' 1852, vol. xv, p. 313.

by bitterness in the liquids, or by the action of sulphuric acid and bichromate of potash on the residue obtained by ether. 3. The lungs, heart, liver, and kidneys. These parts, similarly treated, gave alcoholic and aqueous extracts having no bitter taste. The residue left by ether was in the form of circular white specks, which, when examined by a quarter-inch power, had no distinct crystalline form, but were obviously granular. When tested with sulphuric acid and bichromate, neither the blue nor the purple colour appeared, but after a time a reddish-brown streak formed around the fragment of bichromate of potash. This was examined by a friend, and we came to the conclusion that the result was by far too doubtful to infer the presence of strychnia. That any organic matter could not have obscured the colour was proved by the counter-test of adding to the acid a minute portion of strychnia. 4. The muscles gave aqueous and alcoholic extracts which had no bitterness whatever, and the ethereal residues gave no effects with the tests indicative of strychnia.

Without any knowledge of my experiment, Dr. Christison, in company with Dr. Maclagan, had performed one of a similar kind, the results of which he communicated to me on the 13th of June.

EXPERIMENT 7.—A *quarter of a grain* of pure strychnia, obtained from Morson, made into a pill with bread-crumbs, was thrust down the gullet of a rabbit. There was decidedly no effect for twenty-five minutes. In ten or fifteen minutes more, no one but a familiarised observer would have noticed anything wrong; but when the animal was raised or pushed, its limbs were tremulous and stiff. In about one hour the first spasm occurred; in three quarters of an hour more it was dead, *making one hour and three quarters* from the time of taking the poison until death. This, as Dr. Christison remarks, coincided as nearly as possible with the time of commencement and the time of termination of the symptoms in the case of John Parsons Cook. The body remained quite flaccid for five or six minutes, and then passed through the usual stage and usual degree of rigidity for two days. The right heart, examined twenty-four hours after death, was pretty full of fluid blood,—the left was contracted and nearly empty.

The stomach contained a large quantity of food. This, with the contents, was examined for strychnia by Stas's process, the whole of the steps being followed as described by him. Strychnia was found unequivocally by taste and the bichromate test, in the contents of the stomach.

Not a trace of strychnia could be found by the application of these tests to the liver, treated scrupulously by the same process. Not a trace could be found by the bichromate test in six ounces of the muscles separately examined. The extracts obtained were entirely free from any bitter taste.

Dr. Christison observes in reference to this experiment, which was carefully made, not to support a theory but with a purely scientific object, "I can have no doubt that death may be occasioned by strychnia, in circumstances admitting of ample time for full absorption, and that in the tissues it may be impossible to find strychnia by Stas's method."

These two experiments, with nearly the same dose of strychnia upon the same kind of animal, were conducted independently, and it will be observed that the chemical results were as nearly as possible the same. The sulphate of strychnia employed by me, would correspond to about four tenths of a grain of pure strychnia; therefore, the dose was smaller than in Dr. Christison's case; it was given in perfect solution, while Dr. Christison gave the strychnia in a solid form. The smaller quantity, and the fluid form, would be more favorable to removal by absorption; not to mention that the few drops of solution in my experiment, had to traverse the whole of the gullet of the animal, while the pill would probably reach the stomach in the form in which it was administered. These differences will account for the more rapid supervention of symptoms and death in my experiment, as well as for the smaller quantity of strychnia discovered in the contents of the stomach; but it is perfectly clear from the results, that in a small dose, whether given in perfect solution and destroying life rapidly, or in a solid form and destroying life slowly, an animal may die from strychnia, *and no trace of the poison be found in the tissues.* It may be found in the stomach in larger or smaller quantity, according to whether it be given in solution or as a solid, and whether we have or have not the stomach and its contents in an undamaged state. In my experiment, the animal died in twenty

minutes, and certainly not more than the 100th of a grain of strychnia was separated from the contents of the stomach. Who can doubt, as so large a proportion of the poison had disappeared in this short period, that if the animal had survived an hour, none would have been found in the stomach? Again, if out of 39-100ths of a grain which had thus been lost from the stomach in twenty minutes, not a trace could be found in the heart, lungs, liver, kidneys and blood, is it probable that after sixty minutes, the removal of another 100th of a grain from the stomach would have made any practical difference in the negative results of the analysis? None would have been found in the tissues and none in the stomach.

In Mr. Herapath's experiments, the smallest dose given was four times as great; and in Dr. Wrightson's single experiment on a dog, eight times as great as in these experiments performed by Dr. Christison and myself, and yet these witnesses asserted on oath that, when given in a *minimum* dose to kill, strychnia must always be detected in an absorbed state in the tissues! When this was stated upon oath, to influence the minds of the jury to adopt the theory set up for the defence "no strychnia found,—no poisoning," the witnesses had *not even tried a minimum dose* on animals, and therefore were certainly not in a position to swear to the results of an analysis.

I subjoin a tabular statement of these experiments:—

*Before the Trial.*

No.	Dose.	Commencement of Symptoms.	Total period until death.	Analysis.	
				Stomach and Contents.	Blood and Tissues.
No. 1	1 gr. Sulphate Strychnia, solid.	7 minutes.	13 minutes	Detected by taste only.	Nil.
„ 2	2 gr. Strychnia, solid.	9 „	22 „	By taste and Bichromate.	„
„ 3	1 gr. Strychnia, solid.	9 „	17 „	Nil.	„
„ 4	½ gr. Sulphate Strychnia, solid.	10 „	18 „	Nil.	„
„ 5	½ gr. Strychnia.	12 „	23 „	No analysis.	No analysis.
<i>After the Trial.</i>					
„ 6	¼ gr. Sulphate Strychnia in solution.	15 „	20 minutes	By taste and Bichromate.	Nil.
„ 7 (Dr. Christison.)	¼ gr. Strychnia, solid.	56 „	104 „	„	„

Facts cannot neutralize each other ; and if in fairly adapting their experiments to meet the real question at issue, the chemists employed for the defence could show that under a minimum dose of the poison they can detect strychnia in the tissues of an animal killed by it, still this would not affect the results obtained independently by Dr. Christison, Dr. Maclagan and myself.<sup>1</sup> It is difficult to conceive any number of affirmative instances which would justify them in laying down as an immutable chemical law that in poisoning by strychnia the poison must always be detected. The assumption of superior skill or superior means of research could not be met by any argument ; and, in reference to the search for strychnia, can hardly be claimed by gentlemen who have had so little experience of the various processes for detecting it in the tissues, that one had not sought for it until six days before the trial of Palmer ; and the other had not tried the experiment when he had had ample opportunity, but had seen it performed by another just twenty-four hours before the trial commenced !

Since the occurrence of Cook's case, I have received various communications from scientific men in reference to the detection of strychnia in the blood and tissues. Dr. Ogston, of Aberdeen, informs me, in the case of a man, who died in about twenty minutes after taking a quantity of strychnia (the dose not accurately known) that, besides detecting it in the stomach, he found traces of the poison in about four ounces of blood.

Dr. Macadam has also found strychnia in the blood and tissues of animals, in several instances, both with large and small doses. In one instance, it was detected in the tissues of a cat, to which a quarter of a grain, mixed with a grain of muriate of morphia, had been given,—the symptoms came on in forty-three minutes, and the animal died in fifty-six minutes. In another instance, half a grain was given to a dog,—the urine voided only *nine minutes* afterwards, was found to contain strychnia, a fact showing how rapidly the poison is diffused and passes out of the body. It is remarkable, too, that up to this time, the dog had manifested no symptoms of poisoning by strychnia. In seventeen minutes, another half grain was given ; and, in another minute, tetanic spasms with the usual symptoms came on. The animal was dead in forty minutes

<sup>1</sup> See Dr. Crawcour's experiment, Appendix, p. 152.

from the time of taking the first dose. From the results obtained in another experiment, he has drawn the conclusion that strychnia, taken in small doses at intervals, is absorbed and retained in the system, notwithstanding that it is thus so rapidly eliminated by the urinary secretion. He gave to a horse thirty-two grains of strychnia in divided doses, the animal having in the first instance taken twelve grains. An hour elapsed before the first symptoms were observed; and the animal died tetanic in two hours. He found strychnia in the contents of the stomach, the muscles, the blood, and the urine contained in the bladder, but he did not detect it in the liver, lungs, spleen, kidneys, or heart.

Dr. Cowan, of Glasgow, has furnished me with the results of some experiments lately performed by himself and Dr. Lawrie. Three dogs were poisoned with a quarter of a grain of strychnia; two of these were examined by Dr. Anderson, and one by Dr. Penny. There was unequivocal evidence of the presence of strychnia in the stomachs of the three dogs. Dr. Anderson found traces of it in the liver. Dr. Easton found it in the urine of one of the dogs, while under the influence of chloroform. Dr. Penny examined with great care the brain and spinal marrow of a dog poisoned with strychnia, but failed to discover any trace of the poison.<sup>1</sup>

Mr. Horsley, of Cheltenham, has communicated to me the results of his observations, which were the subject of a paper read at the late meeting of the British Association (August, 1856). In three experiments on cats, to which he had given doses of strychnia, varying from one quarter of a grain to one grain, the animals having died after some hours, he could not detect any trace of strychnia in the bodies. In one experiment on a dog he gave *two grains* of strychnia in a pill with conserve of roses, wrapped in blotting paper. *Five hours* elapsed without any symptom of poisoning showing itself; in the morning the dog was found dead. On inspection, the right side of the heart contained no blood (the auricle and ventricle were empty) while the left side contained blood in a partly coagulated state. When the stomach was opened, the pill was found still enclosed in the paper-wrapper. Mr. Horsley subsequently recovered

<sup>1</sup> Cases of Poisoning by Strychnia, with Experiments, &c. 'Glasgow Medical Journal, part xiv, July, 1856.



from it one grain and a quarter of strychnia, thus showing that three quarters of a grain had been removed by solution and absorption, over a period of upwards of five hours, and had poisoned the dog. The contents of the stomach gave slight indications of strychnia; but no portion of absorbed strychnia could be detected in the blood of the animal, or in the tissues of any part of its body. They were examined both by chloroform and ether; but the results were equally negative. Mr. Horsley sent to me a portion of the blood removed from the body of this dog. In the dried state it weighed 168 grains, corresponding to about two ounces. This was examined by Stas's process with potash and ether, but there was not the slightest indication of the presence of strychnia, either by the taste-test or by sulphuric acid in conjunction with bichromate of potash, peroxide of manganese, or ferricyanide of potassium. This, then, confirmed the result previously obtained by Mr. Horsley. From other experiments which he has performed, Mr. Horsley thinks that strychnia may, in certain cases during absorption, enter into intimate combination with the albuminous parts of the tissues, and thus be removed from the sphere of ordinary chemical analysis. This, however, involves another branch of inquiry. The fact derivable from these researches is, that he failed to find strychnia, in cases in which he knew that it had caused death, and in which he was diligently looking for it.

The conclusions to which it appears to me this inquiry leads, are—

1. That strychnia may be found in the stomach, as in other cases of poisoning, when it has not been entirely absorbed, and the stomach and contents have been properly preserved for analysis.

2. That in some cases, when given in small doses, and in other cases even in large doses, although it may be detected in the stomach (if carefully preserved), it cannot be detected in the absorbed state in the blood and tissues.

3. That there are no facts derived from experiments on animals or from observations in the human subject, to justify the statement that in *all* cases of poisoning by strychnia, the poison must, by proper chemical processes, be certainly detected.

4. That in strychnia-poisoning, as in morphia- and other forms of poisoning, a person may live a sufficient time for the poison to be entirely removed from the stomach, and in this case he may die without a trace of strychnia being found in the blood, tissues, or any part of the body.

*What becomes of the Strychnia in a case of poisoning in which it cannot be detected by chemical analysis?*

This question, as it will be perceived, was much dwelt upon by the counsel for the defence. Under chemical advice, he undertook to give a plain and decided answer to the effect that strychnia was in its nature utterly indestructible, that it could not be lost when once in the body by digestion, absorption, diffusion, decomposition, elimination in the living, or by putrefaction in the dead. In fact, when once in the body, and it had worked as an instrument of death, it was just as indestructible as rock crystal or the diamond, and that the unerring tests of chemistry might in all cases bring it to light. It is only fair to state, that he had not a single physiologist to advise him with respect to the correctness of this opinion, and, for the purpose of the defence, he was bound to believe the statement of his chemical advisers.

The defence was laboured to a most extraordinary extent on this curious physiological question, simply in order to prove that Cook could not by any possibility have died from strychnia. Strychnia, the jury were told, although an organic compound of four elements, carbon, oxygen, hydrogen, and nitrogen, was quite indestructible under any circumstances. It had been sought for by gentlemen who were described as well skilled in analysis, but not a particle could be extracted from Cook's body. If he died of it, it must have been there: and if there, it must have been found.

I have elsewhere remarked, that one strange fallacy pervaded the whole of this reasoning. The only part of Cook's body examined for strychnia, for reasons already assigned, was the *substance of the stomach*, and yet the argument was so put, that the jury were led to believe that every portion of the body had been searched for strychnia, and none was found! In being required to explain, why, when a *minimum* dose of the

poison proved fatal, it might not be found in the body, as in a case like that of Dr. Warner, I stated that the strychnia might be partially changed, and thus withdrawn from the ordinary processes of analysis.

Q. In addition to this distribution of the half grain over the whole system (*i. e.*, in the small proportion of one fiftieth of a grain to a pound of blood), in your opinion, does that undergo decomposition, as it mixes itself with the animal tissues?

A. I believe it *partially* undergoes *some change* in the blood. Q. Would that increase the difficulty of detecting it in the tissues? A. It does. I have never heard of its being separated in a crystallized state from the tissues.

These answers were immediately seized by the counsel for the defence, and applied to Cook's case; but in a most untruthful manner. I had not said that strychnia was *entirely destroyed*, but that it was *partially changed*; statements leading to wholly different conclusions, because, assuming that strychnia has been procured from the tissues and bones in the ready manner in which recent experimentalists affirm, my statement is perfectly compatible with the detection of an *unchanged portion*, while the theory of entire destruction, invented by the learned sergeant, *pro hac vice*, would be quite inconsistent with its discovery in the blood or tissues. To Cook's case, for the reasons stated, this question of partial change could have no application whatever.

I may here dismiss, briefly, another question, which occupied much of the time of the Court, in the examination of witnesses, to no purpose, so far as the case of Cook was concerned,—namely, the supposed loss of strychnia by *putrefaction*. A large mass of scientific evidence was brought forward to show that in solids or liquids highly putrefied, strychnia, if once placed there, might still be found and its properties demonstrated by the colour-tests. The body of Cook had not undergone any material change from putrefaction. He died on the 20th of November (a cold season of the year), and the viscera were under examination on the 28th. There was not the least ground for supposing that putrefaction had interfered in any way with the detection of an organic or an inorganic poison. Dr. Rees and myself, had given no opinion on the effects of putrefying matter on strychnia, and the proved detection of it in dead dogs and in decomposed blood after long periods, was quite unnecessary. I have myself detected strychnia in the

stomach of a dog when in a highly putrefied state, and although I cannot go to the length of the witnesses for the defence, in asserting that this alkaloid altogether resists putrefying changes in the midst of dead animal matter, I believe that it is not easily destroyed by the process. Their experiments do not, however, bear out their conclusions. Not one of them has shown, that after such exposure of strychnia to putrefying matter, the *whole remains unchanged*. Their facts, as far as they go, are quite consistent with partial destruction even by the putrefactive process.

On the other question, of partial change in the living body, a few words may be said. We have no fact to show that strychnia undergoes *digestion* in the stomach. The gastric juice is a powerful agent; but, so far as I know, its action on strychnia has never been determined by experiment. The acids of the stomach dissolve strychnia, and place it in a state favorable for removal by absorption.

The rapidity with which strychnia is absorbed and diffused through the body, must vary according to many circumstances. An experiment of Dr. Macadam's, already referred to, shows that the poison not only may pass into the blood, but that it passes out again by the kidneys in the very short period of *nine minutes*. This may happen, provided the poison comes in contact with the surface of the stomach, and there is no mechanical or other cause to interfere with its absorption. On the fact of its diffusion, there is one set of experiments by Mr. Blake: he found, on introducing the nitrate of strychnia into a vein, that the action of the poison on the spinal cord was manifested by tetanic convulsions in sixteen seconds in the horse, in twelve seconds in the dog, in six and a half seconds in the fowl, and in four and a half seconds in the rabbit. Severe symptoms cannot be produced until the poison is diffused through the circulation; and the more rapidly it enters the blood, the more speedily do the effects appear. This shows how largely absorption must be concerned in the operation of this poison. Dr. Christison killed a dog in *two minutes*, with the sixth part of a grain dissolved in alcohol, injected into the chest, and a wild boar was killed in *ten minutes* with one third of a grain. An instance has been privately communicated to me, in which a man died in *ten*

minutes from a dose of ten grains! This is the most rapid case of death yet known; and there must have been here very speedy absorption and diffusion. Dr. Harley injected one twelfth of a grain of acetate of strychnia in solution, into the jugular vein of a full-grown dog; in *four seconds* the animal became tetanic, and in twenty-eight minutes it died.

In reference to the rapid *elimination* of the poison, we have the evidence of those gentlemen who state that they have found it in the urine. In Dr. Macadam's experiment it was found, as above stated, in the urine of a dog to which half a grain of strychnia had been given on liver, only nine minutes before. The remarkable feature in this experiment is, that no symptoms of strychnia-poisoning had up to that time manifested themselves in the dog.<sup>1</sup> Hence, assuming that there was no fallacy, it follows that the poison begins to be thrown off by the blood before this fluid has acquired a sufficient quantity by absorption and accumulation, to produce symptoms.

This statement is important in reference to poisoning by strychnia, since the collection and examination of the urine while the person is living, might, according to the results of this experimentalist, furnish evidence of the poison being in the system. It also opens to view another question in reference to a supposed *partial change* in the poison, during its distribution through the circulating system. This is what may be called decomposition. Dr. Macadam's experiment clearly shows that strychnia, as such, may be in the blood in sufficient quantity to be thrown off by the urine, and to be detected in that fluid, and yet no symptoms of strychnia-poisoning will exist. To what can this unexpected result lead? It appears to me to be one of two conclusions: 1st, either that the blood is not sufficiently saturated with strychnia to produce the usual effects on the spinal marrow; or, 2dly, that in poisoning by strychnia, a certain time elapses before the blood undergoes such a change by the presence of the poison, as to cause the symptoms peculiar to strychnia-poisoning.

It will be desirable, before speculating on this point, to refer to a few facts regarding the detection of strychnia. In the experiments performed by Dr. Christison and myself, in which a quarter of a grain of strychnia was given to rabbits,

<sup>1</sup> 'Pharmaceutical Journal,' August, 1856, p. 124.

a small quantity, the surplus of that which had gone into the blood, was detected in the stomach, but not a trace in the blood or tissues. In Dr. Harley's experiments, just now related, a dog was killed by the twelfth of a grain of acetate of strychnia, injected into the jugular vein. This gentleman states that he lost no time in making a minute examination of the blood. He employed Dr. Marshall Hall's physiological test for this purpose, but although this is an exceedingly sensitive test, there was no effect; thus showing that there was no strychnia in the blood. The poison, therefore, in these cases, must have been either decomposed, or so diffused that the quantity was too small to admit of detection. Other facts of a similar kind might be quoted. The result of the above experiments is, that when introduced directly into the blood in *minimum* fatal doses, strychnia cannot be found in that fluid, even when death has occurred in twenty-eight minutes. When introduced into the stomach in *minimum* fatal doses, after variable periods, there may be sufficient left in that organ to enable the experimentalist to say that strychnia is present, but not a trace can be found in the blood or any parts of the body. Again, when strychnia has been given medicinally or taken as a poison, it cannot always be found in the urine. (See p. 152.)

What becomes of the strychnia? Is it that there is a limit to the action of the tests, beyond which they cease to reveal its presence, although it is there; or is it to be admitted that it has undergone a change? The theory put forward in the defence, that strychnia is as unchangeable in the animal body as rock-crystal, would, if it were well founded, forbid our entertaining the first supposition. Tests which act up to the 50,000th of a grain upon a substance that is perfectly indistructible, and even resists the action of oil of vitriol, can never be supposed to fail! The only conclusion is, then, that the poison must have undergone some change, owing to which it can no longer be reproduced as strychnia.

There are some facts in support of this view, derived from the recent experiments of Drs. Macadam and Harley. Dr. Harley found that the flesh of animals killed by *minimum* doses of strychnia, did not act as a poison to other animals. He fed a hedgehog on poisoned flesh for a period of fourteen days,

without being able to detect the slightest symptom of poisoning. The poison must, therefore, he concludes, have been either decomposed, or it was not present in sufficient quantity.<sup>1</sup>

I have already had occasion to refer to Dr. Macadam's experiment. He killed a horse with thirty-two grains of strychnia, which can hardly be called a minimum dose. He fed a large-sized terrier dog for two weeks on the flesh of this horse; the animal eating every day during this period two pounds of muscle. The terrier dog, he says, lived and thrived on the flesh, and did not betray the faintest shadow of tetanic symptoms. He states that, on analysis, he found distinct evidence of strychnia in the muscle and blood of the horse.

These results appear to me to favour the view that strychnia, when absorbed and diffused through the circulating system, undergoes some change, by which, although part of it may still exist as strychnia, a portion is so altered or decomposed, that it no longer possesses a poisonous action on animals. Either this view must be adopted, or we must be prepared to say that strychnia, unabsorbed or as it is lying in the stomach, is a poison; but when absorbed and deposited in the tissues, although still existing entirely as strychnia, it ceases to exert a poisonous action.

Does strychnia produce any change in the blood? By some, the dark colour and fluidity occasionally observed in this liquid, are ascribed to the direct effect of this and other alkaloidal poisons. I have frequently examined the blood of animals poisoned by strychnia by the microscope, but could perceive no change in the globules. In a fatal case of poisoning by this substance which occurred to Dr. Ogston, he states that the blood-globules under the microscope appeared swollen, and their outline irregular; but this, he thinks, may have been due to spontaneous changes after removal from the body.<sup>2</sup> In Dr. Blumhardt's case (No. 2 in the table p. 78), it was observed that the blood drawn while the patient was living and labouring under the effects of the poison, presented some peculiar characters. It came from the vein dark-coloured, and of tarry consistency; and there were contained in it a number of minute bladders

<sup>1</sup> 'Physiological Action of Strychnia,' p. 15. This interesting monograph was published in the 'Lancet' of the 7th and 14th June, 1856.

<sup>2</sup> 'Lancet,' April 19, 1856, p. 428.

of gaseous matter, appearing to indicate that it had undergone chemical change.<sup>1</sup>

That strychnia acts as a poison, by producing some change in the properties of the blood, either vital or chemical, is, I believe, a generally received view among physiologists. There is no other conceivable theory which will explain its action as a medicine, or its physiological effects as a poison on the nervous system. To suppose that it alters or affects the blood without in itself undergoing some change, is contrary to the generally admitted doctrines of chemistry. It is also contrary to our knowledge of the chemical properties of strychnia and the alkaloids. The extraordinary changes of colour which strychnia undergoes by contact with sulphuric acid and the peroxides in the application of the colour-tests, are properly ascribed to its combination with nascent oxygen. In taking oxygen from these substances, is it to be supposed that it undergoes no change in itself? Is it in accordance with the laws of chemistry, that A can affect B, without B affecting A? Are the coloured compounds of strychnia and oxygen still in the form of strychnia the poison, or have they undergone a change? The amount of this conversion in the application of the colour-test, is well known to be dependent on the relative proportions of strychnia and nascent oxygen produced from the substances employed.<sup>2</sup> An ignorant or careless manipulator might, it is true, on finding strychnia to be present, affirm that the alkaloid had undergone no change; when, in fact, part of it only was there, and part of it had become converted into other compounds, no longer possessing the properties of strychnia in its pure state. The application of this reasoning is obvious. There is in the blood, nascent oxygen or oxygen in a state of high chemical tension for combination with the carbon and hydrogen of the waste tissues of the body. Strychnia, as it passes into the blood by absorption, must have its molecules split infinitesimally. These molecules there meet with oxygen. Considering the remarkable affinity which strychnia shows for

<sup>1</sup> 'Wibmer Arzneimittellehre und Gifte,' art. "Strychnos," p. 258.

<sup>2</sup> Strychnia readily removes the colour of permanganate of potash, in the cold, by abstracting oxygen. The sulphate of strychnia discharges the colour almost instantaneously. On evaporating the solution, and on filtering it to separate peroxide of manganese, a whitish residue is subsequently obtained, which gives the coloured reactions of strychnia; but, so far as I could judge, diminished in effect, probably by reason of the loss of the portion which had become oxidized.



oxygen out of the body, would it be a matter of great surprise that it should combine with, and remove from the blood a portion of that element on which all vital actions and the proper secretion of nervous force depends? If the oxygen of the blood can combine with the carbon and hydrogen of the tissues, there is nothing to prevent its combination with the carbon and hydrogen of part of the strychnia. It is not necessary that the whole of the absorbed strychnia should be thus changed. The conversion of a part would satisfy the theory, and the altered blood would thus become the poison affecting the motor tract of the medulla oblongata, producing an increased secretion of the nervous force which excites the voluntary muscular system.

But there is another mode in which the blood may be affected. According to Mialhe, strychnia is one of those poisons which forms an insoluble compound with the alkalies of the blood, and it thus affects the organic constitution of this important fluid.<sup>1</sup> Mr. Horsley considers, from his experiments, that strychnia enters into intimate combination with albumen, and that it is thereby so changed, in certain cases, as not to be discoverable in the tissues when it has caused death. I have elsewhere alluded to these experiments (*ante*, page 120). Supposing these views to be confirmed by further observation, it is probable that strychnia, besides its assumed effects on the oxygen of the blood and its action on the alkalies in that liquid, may further influence the condition or proportion of the serum.

Liebig, writing in 1842, says, "With respect to the action of other nitrogenized vegetable principles, such as quinine, the *alkaloids* of opium, &c., which manifests itself not in the process of secretion, but in phenomena of another kind, physiologists and pathologists entertain no doubt that it is exerted chiefly on the brain and nerves. This action is commonly said to be dynamic, that is, it accelerates or retards, or *alters* in some way the *phenomena of motion in animal life*. If we reflect that this action is exerted by substances which are material, tangible and ponderable, *that they disappear in the organism*, that a double dose acts more powerfully than a single one, that after a time a fresh dose must be given if we wish to produce the action a second time; all these considerations, viewed chemically,

<sup>1</sup> 'Chimie appliquée à la Physiologie et à la Thérapeutique,' par le Dr. Mialhe, p. 524, Paris, 1856.

permit only one form of explanation—the supposition, namely, that these compounds, by means of their elements, take a share in the formation of new, or in the transformation of existing brain and nervous matter.”<sup>1</sup>

Liebig, therefore, evidently considers that the alkaloids, under which head strychnia and brucia would be included when absorbed, undergo changes of conversion, and that their poisonous action on the body is really due to the changes thus produced.

As far back as 1824, many experiments on the loss of the alkaloids by absorption and by changes produced in the blood were made by Lassaigne, Dublanc, and other French chemists, the question having excited some interest in consequence of the trial and execution of Dr. Castaign. M. Dublanc gives the following as the results of his various experiments: “When the acetate of morphia has been absorbed, it will no longer be found or produced in a crystalline form from any of the liquids or tissues of the animal body. It appears to me to be demonstrated that, during absorption, this alkaloidal salt is rendered so diffusible that the aggregation of its constituent molecules is destroyed, and I much doubt whether it will be possible to find reagents capable of restoring cohesion to these divided molecules, so as to cause them to reassume the crystalline form. I am led to believe that when crystallized morphia is produced from the liquids of the stomach it is only a portion of that quantity which has *not been absorbed*. With this exception it will be found only dissolved, and appreciable by no test except by the blood-red colour given by nitric acid.”<sup>2</sup>

Although this theory of the partial loss of alkaloids, by absorption and diffusion in the blood, has been before the scientific world for the long period of thirty-two years, and has been widely promulgated by Liebig and his disciples within the last fourteen years, it was described by Sergeant Shee and his chemical witnesses as something quite new, which they had all heard of for the first time at the trial of Palmer! Had any one of his professional advisers been known as a physiologist, or as ever having given attention to physiological chemistry, this statement might have excited surprise. As it is, it shows

<sup>1</sup> ‘Animal Chemistry or Organic Chemistry, in its applications to Physiology and Pathology,’ by Justus Liebig, p. 182, London, 1842.

<sup>2</sup> ‘Considerations Medico-Chimiques sur l’Acetate de Morphine,’ par le Dr Vassal, p. 97, Paris, 1824.

that theories propounded by well-known chemists of repute may be entirely unknown to those who are put forward as "adepts" in chemistry for the instruction of a Court of law. It is not expected that gentlemen of the legal profession should be acquainted with these scientific theories, but it is expected that they should be better advised regarding them than they appear to have been on this occasion.

There are few physiologists who doubt that all poisons act through the blood, that they alter its physical or its chemical properties, manifested by fluidity and change of colour; and that the poisonous substance itself simultaneously undergoes a change. This has been established by experiment in reference to oxalic acid, alcohol, and sulphuretted hydrogen; and, according to the statements of Mialhe, the compounds of arsenic and phosphorus with hydrogen affect the vital properties of blood by the removal of oxygen. As an additional proof, M. Bernard has recently found that the cyanide of mercury is resolved into hydrocyanic acid in the capillary system of the lungs. There is, therefore, reason, from analogy, to believe that strychnia may undergo some change in this fluid. The fact that it can be demonstrated to exist in the blood and tissues proves nothing to the contrary, because this may be a portion of the unchanged or undecomposed poison.<sup>1</sup> The question can only be determined by experiment; and it remains to be seen whether, under a *bonâ fide* minimum dose, the whole of the strychnia which has destroyed the life of an animal can be recovered from the dead body in its original state. This is by no means probable, if we may judge from careful experiments already made with minimum doses of the poison. It certainly was not proved, or rendered even probable, by the evidence given for the defence at the late trial.

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There was practically no defence,—no answer to the charge

<sup>1</sup> Mitscherlich Wöhler and Frerichs have discovered that the essential oil of bitter almonds is oxydized in the system when given in *small quantity*, and is converted into hippuric acid in the urine. When given in *large doses*, however, they found that it escaped *unchanged* into the urine. ('Pharmaceutical Journal,' vol. x, p. 83.) See also experiments by Dr. W. B. Herapath, of Bristol, on the assimilation and destruction of disulphate of quinine in its transit through the body, in the same journal (vol. xiii, p. 219, 1853).

of murder brought against William Palmer by such an overwhelming mass of evidence as was produced at this trial. The "incredibly rash" opinion which the learned counsel charged me with having given "upon the loose evidence of chambermaids, and waitresses, and housekeepers," against the opinion of the medical man, Mr. Bamford, who attended Cook in his last illness, and with no encouragement from Mr. Jones,—was immediately adopted from an examination of the depositions and without any communication with me, by Sir Benjamin Brodie, Dr. Christison, Dr. Todd, Dr. Rees, Dr. Daniell, Mr. Curling, Mr. Solly and others. In dealing with the medical evidence for the prosecution, as I have already shown in several instances, the learned counsel for the defence had an unfortunate failing of misstating and misrepresenting facts of the greatest importance in the case; but he appears to have quite persuaded himself that there was no crime,—that Cook had died a natural death, and that his client William Palmer was perfectly innocent! Thus, in addressing the jury, he said, "I commence his defence, I say it in all sincerity, *with an entire conviction of his innocence.* I believe there never was a *truer word* pronounced than the words which he pronounced, when he (the prisoner) said *Not Guilty* to this charge. Further, I will give you this proof of the *sincerity* with which I declare upon this evidence *my conviction of his innocence*, that I will meet the case of the prosecution *foot to foot at every stage.*" How was this promise maintained? Either by a complete evasion of every material circumstance adverse to his case, or by a misstatement of those medical facts on which the guilt or innocence of the prisoner really turned. No reason was given for the purchase and stealthy procurement of strychnia at such an unseasonable time. Not one of the acts of the prisoner in reference to his interference with the dead body, the analysis, or the correspondence relating to it, received any reasonable explanation. The attempted bribery of the post-boy, with a view to the breaking of the jar containing the viscera, was converted into a mere wish to break the neck or legs of a troublesome old stepfather who was so suspicious, and, as it has now been proved, justly suspicious, of the conduct of his innocent client the prisoner!

If we turn to the theory of the cause of death, irrespective of poison, we find a most inadequate suggestion,—excitement

arising from the deceased winning a race, manifesting itself by violent tetanic convulsions a week after the occurrence, on two separate occasions, with complete recovery in the interval; and this, too, occurring in a young and comparatively healthy man, who had never been subject to excitement or had had any kind of fit previously! Some of the learned counsel's witnesses admitted that the symptoms were consistent with death from strychnia: one, who denied this, said they were consistent with nothing that he ever knew: others, again, assigned widely different causes incompatible with each other. The argument based on the non-discovery of poison in the stomach completely failed; it was not even supported by the evidence of his own witnesses; and an examination of their evidence shows that they had no facts in reference to *minimum* doses on animals, and none whatever in reference to the human subject,—on which to found their opinions.

The last argument in favour of the innocence of his client was certainly of a most extraordinary kind. A letter was read which the prisoner had written to his intended wife about six years previously, and before his marriage,—as a proof of his ardent affection at *that* time; and that he was not, therefore, likely to commit such a crime as that imputed to him by this charge six years subsequently! It was well known, however, to the whole world, that a bill for the murder of that wife by poison had been returned against him; and that, in the event of an acquittal on the charge of murdering Cook, he would have been immediately tried for this crime.

It is unnecessary to carry this examination of the case further. It is, however, a subject of regret that in defending prisoners, some learned counsel are unable to perceive that they should not convert themselves into witnesses and asseverate on their own belief, the innocence of a person whom they are simply retained to defend. Their duty is clearly to see a man *tried according to law and not condemned contrary to law*. A species of wild license, however, is sometimes indulged in by certain members of the bar, where a defence is desperate, which cannot fail in the end to be attended with evil results to society. The denunciation of medical witnesses as conspirators to destroy the life of an innocent man without motive;—the coarse imputations of rashness, ignorance and prejudice, on men who simply perform

a duty—always painful, sometimes dangerous in the nature of the investigations required, but absolutely necessary to the safety of society—reflect disgrace on the mode in which we profess to inquire into truth. Learned counsel, who are members of an honorable profession, should bear in mind that the medical profession is equally honorable; and that its members have a claim to be treated with proper respect. A case of poisoning by strychnia may possibly occur hereafter under circumstances more nearly touching the private feelings of the counsel who defended Palmer in this case, than the death of the unfortunate John Parsons Cook. A medical man may be called in. Is it to be supposed that he will easily forget the charges of “incredible rashness” and “unheard-of indiscretion,” brought against witnesses who, in Palmer’s case, only performed a public duty,—even although the verdict of a jury and the voice of the country subsequently confirmed the correctness of their views? Will the learned counsel then be inclined to receive an opinion based, as it may be, upon the statements of chambermaids and waitresses, instead of a committee of surgeons or physicians who cannot always be present to witness an act of murder,—or will he again reject it as rash and indiscreet? The course pursued on this occasion by the learned sergeant, will, I fear, have the evil effect of stopping the expression of such a free and independent opinion as every medical man so situated not only ought to give but to be protected in giving. It will cause him to look more closely to consequences as affecting himself and his future position. The mode of dealing with the witnesses for the Crown, in Palmer’s case, will show him that in fearlessly performing his duty to the country he cannot rely on receiving protection from violent, unjust, and unmerited attacks.

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#### APPENDIX.

##### APPEARANCES OBSERVED IN DEATH FROM TETANUS.

MR. POLAND has kindly communicated to me the following valuable statistical results, obtained from an examination of 68 cases of tetanus recorded at Guy’s Hospital since the year 1825.

63 traumatic—57 died; 6 recovered.

3 questionable as to exposure or injury—2 died; 1 recovered.

2 idiopathic—1 died; 1 recovered.

*Inspections.*—Of these, only 27 were inspected, and the heart noticed in 14; of which, 13 were traumatic, and 1 idiopathic. All died during the paroxysm; there was no case of examination where death took place from exhaustion.

*The mode of dying* of the 60 cases, as far as can be relied upon by the eye-witnesses were—

8 from exhaustion.  
24 during the paroxysm;  
28 not noticed.

*State of the heart in the 14 cases—*

- 1, nothing remarkable.
- 1, stated to be large.
- 1, right side flaccid, and left firmly contracted.
- 1, right side slightly loaded.
- 3, right side full of coagulum.
- 6, both sides contained blood; one of these cases *idiopathic*:
  - In 1, the blood was dark and fluid, and full in quantity.
  - In 1, right side more than usually distended with dark fluid blood; on the left side the same, but smaller in quantity.
  - In 1, the right side had a firm coagulum, and the left some soft clots.
  - In 1, firm clots on both sides.
- 1, heart violently contracted (laryngotomy performed in this case).

At page 44, some remarks were made on the fulness or emptiness of the heart as an indication, or the contrary, of strychnia-poisoning. In death from strychnia, the cavities of this organ have been sometimes found empty, and at other times full; and the facts collected by Mr. Poland from the records of Guy's Hospital show that there is no uniform or constant condition of this organ in fatal cases of ordinary tetanus. Generally speaking, more or less blood has been found in the heart; and in a recent case communicated to me from another source, blood was found in this organ. It is worthy of remark, that in the single fatal case of idiopathic tetanus, blood was contained on both sides of the heart; and in one instance only the heart was found strongly contracted. The empty condition of the heart in the case of Cook was, therefore, more adverse to the theory of death from tetanus, whether of idiopathic origin or as a result of ulcers, than it was to the theory of death from strychnia. In fact, the tabulated cases show that in strychnia-poisoning, emptiness of the heart is an occasional appearance, while in tetanus as a disease it is very rare. It is highly probable that the condition of this organ varies with the mode of death, whether the tetanus arise from disease or poison.

Since these remarks were written, I have had an opportunity of reading an account of the experiments of Dr. Pavy, published in the Guy's Hospital Reports. The results show that strychnia has no direct action on the heart, and that in animals the arrest of the functions of this organ is the indirect effect of the suspension of respiration by spasm of the respiratory muscles. The occasional emptiness of the heart may, Dr. Pavy thinks, be accounted for by some slight contractions of this organ taking place subsequently to the death-spasm, as a result of a few automatic efforts at respiration. It is not so much with the cause, as with the fact that a medical witness has to deal. The cases of strychnia-poisoning tabulated at p. 78, show clearly, whatever may be the cause, that the absence of blood from the

cavities of the heart is certainly not inconsistent with death from strychnia, as the witnesses for the defence wished to make it appear. Whether asphyxia is or is not in all cases the immediate cause of death in human beings poisoned by strychnia is a question purely physiological. It had no practical bearing on the case of Cook, and I have not, therefore, considered it necessary to make it a prominent subject for discussion in this paper.

In addition to the cases referred to at page 44, in which, in death from strychnia, the heart was found empty, another case of poisoning by strychnia, communicated to me by Dr. Ferguson, will be found below (page 142), showing an empty state of this organ. Dr. Norman Chevers, in his excellent Report on Medical Jurisprudence in the Bengal Presidency (1854), in remarking on the appearance presented by the heart in death from drowning, observes, that in cases of asphyxia one or both cavities may be found empty. It is a well-known fact, that in asphyxia the heart continues to beat after the lungs have ceased to act; and Dr. Chevers refers to a case of poisoning by opium, in which, by the aid of galvanism, the action of the heart was maintained for a considerable period after natural respiration had ceased. The contractions of the organ thus forcibly propelled the blood into the lungs, where it could not be oxygenated. On inspection, the right cavities of the heart were found well contracted and almost empty. In a fatal case of traumatic tetanus, which occurred at Guy's Hospital in February, 1856, it was noticed by Mr. Daniell, who has furnished me with the particulars, that the pulse beat many times after the patient had ceased to breathe. No inspection could be obtained to examine the state of the heart in this case.

In the last edition of my work on 'Medical Jurisprudence,' p. 681, I have reported a case of asphyxia from drowning, in which the heart was very flabby, and contained scarcely any blood. The asphyxia produced by strychnia may present the same variable states of the heart as asphyxia from any other cause; and the probable explanation is, that where the heart ceases to act simultaneously with the lungs, its cavities, particularly on the right side, may be found distended with blood; but where slight contractions of the heart go on subsequently to the stoppage of respiration, the cavities may be found more or less completely emptied. We have no means of determining, as Dr. Chevers remarks, for how long a time after apparent death the human heart naturally continues to retain a certain amount of nervous and contractile power; but there is reason to believe that the period is not precisely the same for any two individuals. Some persons asphyxiated by drowning have recovered, under treatment, after five minutes' or longer submersion: while others have not recovered when the submersion had not continued more than two or three minutes.

In the drowning of *animals*, the right cavities of the heart, as in strychnia-poisoning, are generally found distended. This may be owing, in both cases, to the heart's action ceasing simultaneously with that of the lungs. This fact, probably, furnishes an explanation of the difference in strychnia-poisoning which is observed in the state of the heart, not only among human beings, but between human beings and animals.

The 27 inspections collected by Mr. Poland furnish us with additional facts of interest in reference to certain questions which have been examined in this paper.

*Duration of the cases.*—The periods at which the disease proved fatal varied from four or five hours in one doubtful case, to twenty-eight days, this being the longest



period from the commencement of the attack in which the disease proved fatal. The single case of idiopathic tetanus terminated fatally in ten days. The periods were as follows: 1 proved fatal in 40 hours; 1 in 50 hours; 2 in 2 days; 2 in 3 days; 5 in 4 days; 2 in 5 days; 2 in 6 days; 4 in 7 days; 2 in 10 days; 1 in 12 days; 1 in 14 days; 1 in 21 days; 1 in 22 days; and 1 in 28 days.

I subjoin a brief history of the case which proved fatal in four or five hours. It can hardly be denominated a case of tetanus.

"Female, æt. 9. Burn over upper part of body and both arms. Progressing favorably until ninth day of residence in the hospital, when the limbs became suddenly and rapidly rigid, and the deglutition exceedingly difficult. *There were no convulsions.* Death in four or five hours. The brain and cord were found healthy, but firm. The heart had firm clots on both sides."

*Condition of the Brain and Spinal Cord.*—Among the 27 inspections there were 14 in which the brain and spinal cord were *healthy*. Of the remaining 13 cases, the cord was found softened in 2; one of these having proved fatal in ten days, and the other in four days. In 4 cases the cord was congested, these having proved fatal in fifty hours, in two days, in three days, and in seven days respectively. In 2 cases the cord is described as darker than natural; one having proved fatal in fourteen and the other in two days. In the latter case the cord was firm. In 4 the cord was found pinkish, and either natural or firmer than natural; and in 1 case, which proved fatal in four days, the examination having been made forty-one hours after death (in August, 1855), the cord was decomposed, while the body was still rigid.

No granules or deposits were found in any one case. The reader will perceive from this statement of facts observed in the human subject, that there is no uniform appearance in the brain or spinal marrow attendant on death from tetanus. In the majority of cases these parts were healthy, the inspections having been recent. Let these facts be contrasted with the suggestion made in the defence of Palmer to the effect that had a recent inspection of the spinal marrow been made in the case of Cook, a (natural) cause of death might have been revealed!

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#### CASES OF POISONING BY STRYCHNIA.

##### *Death from a large dose in thirty-five minutes—Extreme rigidity.*

For the subjoined case I am indebted to Dr. Christison. It occurred to Dr. Adams, of Banchory. This gentleman says—"About 9 o'clock on the evening of the 9th of May, I was called to see a country lad, about 25 years old, who was said to have swallowed a large quantity of strychnia, procured for the purpose of killing a fox. To the best of my information, I reached the spot about seventeen minutes after the poison had been swallowed, and found him in great agony, lying in bed, and held by his father and another person. He was in a state of intense tetanic convulsion, with his body, from head to foot, nearly straight, except the chest, which was rather arched; his arms and hands were strongly contracted. He was in a most profuse sweat, his face somewhat livid, his lips swollen; he was quite conscious, and articulated with great earnestness, "Strychnine—great quantity; you cannot save me;" or words to that import. When asked as to the seat of his greatest suffering, he complained particularly of the rigidity of his legs. With some difficulty, I managed to

pour down his throat a strong solution of sulphate of zinc, and afterwards a considerable quantity of ipecacuanha powder, mixed with warm water. Assisted by us, he then succeeded in turning himself on the fore part of his body, with his head and shoulders reclined over the edge of the bed; and in this position made several convulsive efforts to vomit, but with little or no success. Having then remarked that he returned no answer when addressed, and feeling his body becoming cold, I turned him back into the bed, and immediately afterwards he ceased to breathe. According to the statements of the bye-standers, he may have lived about *thirty-five minutes* from the time he swallowed the poison. The quantity of strychnia procured by him was *eight grains*, and there is every reason to believe that the whole was swallowed. From what I could collect, alarming symptoms came on very soon, but not instantly after taking the draught. He was alone when he took it, but immediately communicated to his parents what had happened, and he seemed anxious to preserve his life.

On the *third day* after death, when I inspected the body, it was *inflexibly rigid*, especially the lower extremities, the posterior tendons of which were so contracted as to give the foot the shape of *talipes equinus*; the eyelids were open, pupils dilated; the neck in front and all the recumbent parts of the body were of a livid colour. I was not afforded an opportunity of inspecting the internal parts.

The next case occurred in the practice of Mr. Blacklock, of Dumfries. It represents the smallest dose which has yet been known to prove fatal, and illustrates the tremendous power of this poison.

*Death of a child in four hours from the sixteenth part of a grain  
of Strychnia.*

A gentleman, affected with paralysis, had been taking, twice or thrice a day, a pill containing a sixteenth of a grain of strychnia, without any particular effect being produced. One day his son, a child between two and three years of age, had been allowed to get possession of the pill-box, and was seen to put a pill into his mouth; but some doubt existed whether he swallowed it or spat it out. In half an hour, while seated on a chair, he was *suddenly* seized with a twitch or start, and fell from the chair; but his mother, supposing he had done so in play, at first took no alarm. In a few minutes the same happened again, and the child began to cry; upon which the medical attendant of the family was sent for. On examining the pill-box, it was found that one pill was wanting. An attempt was made to induce vomiting by sulphate of zinc, but it failed. Tetanic spasms commenced ere long, but the time of their first appearance is uncertain. Mr. Blacklock, who was then sent for, saw the child about three hours and a half after the pill was taken. He found the tetanic paroxysms recurring with intervals of complete relaxation. A warm bath was now prepared; and, while the child was being lifted into it, another medical gentleman poured a little cold water upon the head. Instantly a violent fit of the usual opisthotonos arose, which almost instantaneously terminated in death. Complete relaxation immediately ensued; strongly reminding Mr. Blacklock of the fatal termination of a case of traumatic tetanus which he had witnessed several years before. The child's body being placed in bed, lay flat on the back; not more than *four hours* having elapsed from the time the pill was swallowed.

The box of pills was a fresh supply of sixteen, of which the gentleman had taken one. Fourteen were found in the box. The pills had been made up in the establish-

ment of the principal druggist in Dumfries, and by the master druggist himself, a man of experience and skill; an error is thus rendered highly improbable; and so the fatal dose is pretty accurately fixed at the sixteenth part of a grain!

Dr. J. B. Nevins, of Liverpool, informs me that he has known one sixteenth of a grain of strychnia produce such violent tetanic cramps, that the patient could not be induced to take a second dose.

*Poisoning by the twentieth part of a grain of Acetate of Strychnia—Sudden accession of symptoms after an hour—Recovery.*

A physician of Liverpool thus writes:

In July, 1851, I was induced on the recommendation of a medical friend to give strychnia to my little boy, then five years old, in consequence of a variety of remedies having failed to cure him of irritability of the bladder. It was given as acetate in the twentieth part of a grain, at my bedtime; after the first dose, *an hour or thereabouts* had elapsed, when he awoke screaming, jumped up in bed, and was much excited and alarmed; the muscles of the face were much contorted, and the entire body thrown into a state of opisthotonos. After about ten minutes' convulsive movements, the stiffness decreased, and he sank into a most uneasy sleep, with slight twitchings of the muscles during the remainder of the night; he was very weak and feeble the next day, but no bad effects remained permanently as he is alive and well at present.

*Fatal dose in an adult—One half grain of Sulphate of Strychnia—Death in twenty minutes.*

The case of Dr. Warner is one of peculiar interest. A very brief reference is given to it at p. 81. The following account from the 'British American Journal' for August, 1847, completes the history.

At a meeting of the Addison County Medical Society of Vermont, the undersigned were appointed a committee to ascertain the facts in the case of one of their members, the unfortunate William Cullen Warner, M.D., of Bristol, who deceased, suddenly, at Montpelier, October 11th, 1846, in the thirty-ninth year of his age, while he was a member of the Legislature.

On account of there having been considerable discrepancy in the published reports in relation to this melancholy event, the committee addressed letters of inquiry to the Hon. Daniel O. Onion, M.D., of the Vermont Senate, and to Charles W. Horton, M.D., Member of the House, each of whom, they had learned, were present during most, if not all, the period of the sudden and tragical event. To the inquiries of the committee each of these gentlemen has given prompt and satisfactory replies, which in substance are here subjoined.

1. *In your opinion how much sulphate of strychnia was taken?*

To this Dr. Onion answers, "I think probably *from one fourth to one half a grain*. As he intended to take, and supposed he was taking, morphia, he would be likely to use the same quantity he was in the habit of using of that article, although there was no evidence at the time of the quantity taken." To Dr. Horton, who was called into the room immediately after the accident, Dr. Warner said, "Doctor, I have taken by accident an over-dose of morphine; help me if you can;" at the same time handing him the phial enveloped in paper.

2. *How soon after was any effect produced?*

Dr. Horton says, "It is my opinion, from facts subsequently obtained from Gen. W. Nash, who occupied the same room with him, that he felt the effects in less than five minutes."

3. *What was the first symptom?*

Dr. Horton replies, "*Constriction of the throat and tightness of the chest, with rigidity of the muscles in attempting to move.*" Dr. Onion says, "He first complained of a want of air, and requested the window to be raised; whether it was from faintness or a constriction about the respiratory organs, I do not know, although I think the latter."

4. *What symptoms ensued from the first till death occurred?*

Says Dr. Onion, "When I first saw him, he was lying upon the bed in a complete tetanic convulsion; his head somewhat drawn back; his countenance completely livid, with some frothy matter issuing from his mouth, with frequent moans. The palpebræ were constantly in motion. This first paroxysm may have lasted some five minutes, which was succeeded by an interval of partial calm." "During this interval," continues Dr. Onion, "it was somewhat difficult for him to articulate with distinctness. He made several attempts to vomit in this interval, by exciting the fauces with his finger. There seemed to be some constriction about the throat, as it was difficult for him to swallow." "This interval lasted perhaps five minutes, when another paroxysm commenced by a little starting and stiffening of the extremities, and immediately the whole body was thrown into a tetanic paroxysm, in appearance like the first, and lasted two or three minutes, when death ended the struggle."

"In about three minutes from the first paroxysm," says Dr. Horton, "the tetanus again returned, and in the space of two minutes death closed the scene, with terrible spasms of the entire system. The pulse remained unaffected till the last struggle. It is my opinion that the immediate cause of death was exhaustion (?) from spasm."

"His appearance," says Dr. Onion, "led me to believe that death ensued from asphyxia or suffocation. There must have been great congestion of the brain, which of itself might have proved fatal."

5. *How soon after taking the article did death occur?*

Dr. Horton says, "From the best information which I could obtain, I should judge that death ensued in fourteen minutes." "The time from taking the article till death ensued," Dr. Onion remarks, "could not have been over twenty minutes."

6. *Did his mind remain clear till the last struggle?*

"I think," replies Dr. Horton, "that he was perfectly conscious from the first to the last, except in the paroxysm of tetanus, from the following facts:—1. His appeal which he made to me, as noted in the first article. 2. On loosening his cravat, he requested me to unbutton his vest, at the same time desiring me to take out his gold watch and take care of it. 3. An emetic having been administered, he applied his finger to his throat to provoke a nausea. 4. And, from the last words he uttered, 'I fear, I fear, O God, deliver me.'"

7. *What means were used to prevent the fatal result?*

Dr. Horton says, "On witnessing the first symptoms, I left the room for the purpose of obtaining medicine. I procured an emetic of sulphate of copper and

ipeacac. ; but returning and finding him in a tetanus, I immediately dashed cold water on his head, face, and breast, and used the most powerful friction on the extremities. He returned to a state of perfect consciousness. I then proceeded forthwith to administer the emetic, making use of diluents copiously. I sent a messenger for some vinegar and ground mustard, and another for a stomach-pump. I used the ground mustard, in warm water freely, to all of which the patient submitted, seeming to be very grateful for the efforts which I was making for his relief. The means were used without any apparent effects." "When death had ensued, a number of the medical fraternity being present, we retired into an adjoining room, when the fatal bottle was produced, with the wrapper still around it. On removing this, it was found labelled 'strychnine.'" Dr. Onion states, that "till this time, we were in ignorance of what he had taken." Dr. Horton avers, "that here I wish definitely to state, that before the last paroxysm came on, I was fully convinced in my own mind that the fatal drug was not morphia, but strychnia, and I so declared to those present at the time."

From facts before the committee, derived from reliable sources, it appears that on the afternoon of the second day before the fatal accident, Dr. Warner called at an apothecary-store in Montpelier, and asked for and purchased what he supposed to have been a bottle of sulphate of morphia. This was handed to him by the apothecary, enveloped in a brown paper and twisted at both ends. That on the fatal morning Dr. Warner tore off the envelope surrounding the mouth of the bottle, and took a portion of what he supposed to have been morphia. He then proceeded to pour some of the supposed morphia into a small phial in which he had been in the habit of carrying sulphate of morphia, when he was suddenly arrested by the symptoms narrated. It is quite clear that he never entertained any idea of the fatal drug he had taken. "I am certain," says his afflicted brother, "that he never for a moment suspected that he had taken strychnia, and was wholly unconscious of the agency which had produced his awfully unprecedented sufferings."

Dr. Warner had never possessed very firm health, and for about two years before his death he had suffered from an inordinate action of the heart, for which he had occasionally taken morphia. This affection of the heart had been the sequence of an inflammatory affection of the chest, which he had early in the year 1844.

The committee have taken considerable pains to ascertain the facts in this melancholy instance of death from a mysterious mistake. The mistake was certainly a singular and mysterious one, both in relation to the apothecary and the unfortunate man. It appears that Dr. Warner asked for sulphate of morphia; the apothecary intended and supposed he had sold him morphia till after the fatal event, when he found, through mistake, he had given him, enveloped in a paper, a bottle of sulphate of strychnia in lieu of morphia. This exposition of facts appears to be demanded in justice to the character of the deceased, to the apothecary, and to the medical profession.

In a medical point of view the case is one of much and deep interest, since it so clearly manifests the true and energetic character of this somewhat new medicinal agent. And in a medico-legal consideration, it may prove of immense importance. In the suddenness of the effects, and in the quickness of the fatality, from the use of strychnia, this case is probably without a precedent. Christison, Pereira, and several monographical writers in the periodicals, have recorded some bad results and some fatal cases, from over-dosing with this agent; but no instance has fallen

under our notice in the human subject in which its administration, either accidentally or otherwise, has so speedily and terrifically proved fatal.

JONATHAN A. ALLEN, M.D.

ERASMUS D. WARNER, M.D.

WM. P. RUSSELL, M.D.

*Death from a full dose of Strychnia—Symptoms in a quarter of an hour—Intermission—Sensitiveness of the skin—Fluidity of the blood—Emptiness of the heart, with relaxation.*

The following case, for the particulars of which I am indebted to Dr. John Ferguson, occurred at Kingston, Jamaica, in 1846.

A young man, aged about 30, of respectable family, in character reckless and profligate, in a fit of rage, after having threatened to destroy himself, mixed a portion of strychnia with water, in a tumbler, and returning to the room where his mother was grieving over his conduct, and telling her he would now convince her that he was in earnest, he commenced drinking from the tumbler. A young lady, who was present, succeeded in striking the tumbler from his hand, but not before he had swallowed some of the strychnia. I arrived in about *ten minutes* (perhaps) after this had occurred. He was sitting in a chair, still angry, and quite remorseless. He told me he had taken strychnia; he said about *three grains*, and that he had mixed in the water *eight grains*. I have reason to think he knew how much he had mixed; but of the quantity swallowed, he could form but a vague idea. The tumbler was standing on the table, and there was some white powder both inside and outside of it, and some also had been spilt with the water on the floor. He seemed so free from all the effects of strychnia, when he was telling me what he had done, that I had strong doubts of his veracity, and had begun to think he had not swallowed any, but was anxious to keep up alarm. He spoke as if he thought he had failed in his design, for he said he should take care that the next dose should be strong enough. In not more than five minutes from the period of my first seeing him, making probably *a quarter of an hour* from the time of his taking the poison, the effects began to appear. His ferocious look had disappeared during our conversation, and he was becoming calm and tractable. He suddenly said, "It is coming on;" and I noticed he had twitchings about the left side of the mouth. I hastened, and had him removed upstairs to his room, and laid on his bed. The spasmodic action now came rapidly on. First, there was a sudden start of the whole body; then quick contractions of the muscles of the neck and back, drawing back of the head. On raising the body at this moment, with the view of making him swallow an emetic, the back and head were drawn back by strong spasmodic action of the muscles, and those muscles at the same time making quick, short contractions. The muscles of the throat were rigid, and also making strong, quick contractions. Not a drop of anything could be swallowed. When these more partial contractions had continued for a short time, with their intervals of complete cessation, strong general convulsions came on involving the whole body, but deriving, I think, a character from their shortness, quickness, and sharpness. After two or three of these general convulsions, the breathing ceased; the pulse could not be felt; the face, before livid, became pale; there were some horrible distorting movements of its muscles; the corners of the mouth were drawn down; the eyes were open, and

the pupils much dilated; froth was issuing from the corners of the mouth. I thought death had taken place, and was about to close the eyes, when this seemed to be effected by a spasmodic action of the orbicularis muscles.

While looking at this sad spectacle, the muscles of the face again began to twitch, and the respiration returned, but with long intervals; the pulse, too, at the wrist was again perceptible. The spasmodic action had relaxed generally. Whilst he was in this feeble state, I got him to swallow strong brandy and water in spoonfuls. He resisted taking ammonia, and this was no time for any mental or corporeal irritation. He now remained for about half an hour perfectly free from any spasmodic action of the muscles; the respiration became tolerably regular and easy, though he was constantly calling for more air; the pulse more regular, but quick; the heart was very hurried, and palpitating in its action; the colour came again into the face, and its expression was natural. He went on, however, speaking in an excited manner, complaining of want of air and thirst, and constantly asking if I thought he would recover. At the expiration of about half an hour he had again a rather sudden spasmodic starting, as of the whole body, which was repeated once or twice; then these startings were rapidly repeated; then one or two severe convulsions, with the same appearance of the face and eyes as before described, and he died during the presence of one of them; the spasmodic action and life apparently ceasing together.

Touching the eyelid, touching the lip with a spoon, waving a handkerchief near the face to brush away the flies, and at last, so excitable had the muscular system become, that touching the wrist, feet, or forehead, brought on the spasmodic movements. Just before the decided spasm *first* came on, the patient was in the act of telling me that he felt a rather pleasing mental excitement from the strychnia, and his countenance for a few seconds was expressive of that state.

Immediately after death the face was free from distortion and placid; the muscles were by no means rigid; the hands and arms were easily relaxed, and placed by the sides. Blood, with frothy saliva, came from the corners of the mouth.

Next morning after death I inspected the body. The hands were rigidly contracted, and the feet arched and strongly flexed, *i. e.*, retracted on the legs. There was a great deal of sugillation. The blood of the body was *fluid* and *dark*. The vessels of the brain were tinged with black blood, which they contained generally, but more so towards the occiput, from its position. There were apparently patches of lymph, but not of recent deposit, under the arachnoid on the superior surface of hemispheres. *The heart was empty, soft, and relaxed.* The lungs were congested, especially posteriorly, with black blood, presenting more the appearance of a large, dark coagulum of blood, than the texture of the lungs. The stomach had some mucus on its surface, and the mucous membrane of the upper arch and cardiac extremity had red erythematous patches thickly studding it. The veins of the omentum and stomach were large. The liver was healthy. The spinal column was not examined.

The subject of this case had, in earlier life, been apprenticed to a surgeon, and, although he had remained with his master but for a short time, he had acquired some knowledge of medicines.

This case occurred in Kingston, Jamaica, in 1846; and was entered in my note-book the day following that of death.

In my note-book I find the following remark: "It is very remarkable, that after the convulsions or muscular contractions had continued for some time, and had apparently arrested the functions of life, these should again proceed, after

gradually returning; that the spasmodic actions should remain suspended for about half an hour, and then come on as they had done in the first series of symptoms, and present precisely the same phenomena;" the second exhausting entirely the nervous power on which the movements of the organs of respiration and circulation depend; the first not doing so permanently, but yielding to the law of rest, until accumulation should again subject the muscles to the power and action of strychnia.

*Two cases of poisoning by Strychnia, in which one grain was taken—Variable effects of the same dose on different persons—Vomiting—Recovery.*

The patients, in these two cases, were under Dr. Watson's care, in the Middlesex Hospital. They were suffering from paraplegia. A pill containing one twelfth of a grain of strychnia, to be taken every six hours, was prescribed for each patient, one grain of strychnia being divided into twelve pills. By mistake, a *grain* was administered in each case, at seven o'clock in the evening. At 7:30 one of the patients was *suddenly* seized with tetanic spasms: his legs were separated widely from each other, and rigidly extended, and his head and trunk were bent backwards in a state of opisthotonos. His abdomen was quite hard, and his limbs were stiff, even when the violence of the paroxysms had abated. He cried out with the pain at the coming on of these spasms. Any attempt at movement, even the touching of him by another person, brought them on. This, Dr. Watson remarks, is just what happens in the disease. Very slight causes renew the spasmodic tightening of the affected muscles. The patient spoke of a particular sense of constriction all over the abdomen, as if it were drawn in. His intellect was quite unaffected. He had two extremely violent attacks, in which it was thought he would have died. From 8:30 to 12:30 he had several slighter and shorter fits, which left him weak and exhausted, but he soon recovered.

The other patient, who had taken an equal dose of the same preparation of strychnia, did not suffer in the same degree. He said that for a short period he felt dizzy and trembled all over; and at the time when he was visited, he had a weight or uneasy sensation in the nape of his neck, which drew his head backwards, and he experienced some difficulty in opening his mouth and articulating his words. The symptoms were then diminishing, and he was perspiring profusely. A full dose of purgative medicine was given to him, *which acted as an emetic*, and after he had vomited the unpleasant sensations about his head and neck left him.

Another patient at the hospital was attacked with opisthotonos after taking *half a grain* of strychnia. Brandy and water were given to him, and he recovered without having another paroxysm. These cases are recorded by Dr. Watson, in his 'Practice of Physic,' vol. i, p. 553.

*Poisoning by Strychnia—Vomiting—Delay in the appearance of the symptoms—Use of the stomach-pump—Recovery.*

The following case has been communicated to me by Mr. Tubbs, of Upwell. A servant girl took, about seven a.m., a small quantity of a mixture of lard and sugar containing strychnia, which had been laid for the purpose of destroying mice. After licking the poison, she ate a piece of cake to remove the bitter taste. Within five minutes she vomited. She repented of the act, and informed her master. Medi-



cal assistance was called in, and Mr. Tubbs saw her an hour and ten minutes after she had taken the poison. She was then in bed, convulsed; the legs rigid; the pupils natural; the pulse only 74, and weak; the tongue tremulous, and there was difficulty in speaking. The stomach-pump was used, and the stomach washed with milk. After this, there were a few twitchings of the muscles of the face and left arm, and the patient being greatly exhausted, carbonate of ammonia was prescribed; there was for some time restlessness, with difficulty of breathing. The next day she recovered.

*Poisoning by one quarter of a grain of Acetate of Strychnia—Symptoms in five minutes—The power of swallowing—Sensitiveness of the skin—Recovery.*

The subjoined letter was sent to me by Mr. Ingram, of Boston :

“Palmer’s case having drawn the attention of the whole of the medical profession to the subject of poisoning by strychnia, perhaps you will forgive me for mentioning a case of my own, which seems to show that it is a mere matter of time or strength of dose, whether the skin be over-sensitive, and also whether the person can or cannot swallow.

“A patient of mine had for some time taken the acetate of strychnia with great benefit. On leaving home for change of air, he was taking the one eighth of a grain for a dose; he took a supply with him, leaving a small portion that he had not finished in a bottle in his room. A few days after, the servant feeling unwell, and thinking the medicine a simple tonic, took two drachms in water, on an empty stomach, amounting to one quarter of a grain of acetate of strychnia. In five minutes, a general numbness of the whole body came on, beginning in the feet, followed immediately by a sensation in the throat as if it were closing up. At this time, her fellow-servant attempted to put her feet in warm water; this she could not bear, not even the touch of the hand when attempting to remove her stockings. A mustard draught was then attempted to be given; this she could not swallow. Severe cramps in the legs, with twitchings and stiffness of the whole body, now began; there was also a very anxious countenance, and a feeling of great tightness across the chest, more particularly about the heart. On my arrival, within half an hour after the poison had been taken, I found she could swallow in small quantities. I gave her ammonia and water. She could also bear to be touched, and begged to have her legs rubbed to relieve the cramp, which came on by fits about every three minutes. She could not lie down for some time. The symptoms gradually abated, and she could have got up in the afternoon, but felt so sore all over her, that she kept her bed until the next morning.”

The fact that the poison was taken in a perfectly dissolved form, accounts for the very rapid accession of the symptoms in this case. It shows another important circumstance in reference to the evidence for the defence, namely, that in one and the same case, the loss of the power of swallowing and great sensitiveness of the skin may present themselves as symptoms at one stage, and be entirely absent at another.

*Poisoning of a child by one grain of Strychnia—Death in half an hour.*

Mr. Clack, of Eton, has given to me the particulars of a case, in which a child, between three and four years of age, took by an unfortunate accident twelve pills containing in the whole *one grain* of strychnia, mixed with conserve of roses. The usual symptoms came on, and the child died in half an hour.

*Strychnia—Time at which the symptoms appear in man and animals.*

At p. 42 some remarks have been made on the objection taken by certain witnesses, in the defence, that the death of Mr. Cook was not owing to strychnia, on account of the long interval which must have elapsed (one hour to one hour and a quarter) from the time at which the poison was assumed to have been administered and the occurrence of the first symptoms. Although the incorrectness of this view has been proved by the cases referred to in the text, it is desirable to place on record any additional facts, with a view to the prevention of similar mistakes in future. My friend Dr. Geoghegan, of Dublin, informs me (October 6th) that he has just met with a case of poisoning by strychnia, in which the symptoms were much longer in appearing than in the case of Mr. Cook. The patient took in jam a portion of a powder used for the destruction of vermin, at eleven p.m. This, by analysis, was proved to consist of strychnia with colouring matter. He awoke suddenly with spasms, at three quarters past one in the morning, thus making an interval of *two hours and three quarters* before the symptoms appeared.

The experience which might, in the case of Cook, have conducted a jury to an erroneous verdict, was derived from animals, among sixty of which, Mr. Nunneley states, the longest interval of symptoms was *thirty minutes*, and the average five or six minutes after giving the poison. The experiments referred to in the text show that Mr. Nunneley's animals must have been singularly susceptible of the effects of strychnia. Dr. Letheby's longest observed interval in animals was *three quarters of an hour*.

Out of a large number of letters which I received at the period of this trial and subsequently, some contained facts which will show the danger of placing reliance upon evidence of this description.

1. Dr. Geoghegan states that, in a cat poisoned by half a grain of strychnia, no symptom, except drowsiness, appeared for *eight* hours. The animal died tetanic, with violent *screaming*, in two hours more.

Mr. Holman, of Hurstpierpoint, furnished me with the fact recorded in the sub-joined letter:

2. On Friday, May 2d, a small dog of a mixed breed, between a Skye terrier and a poodle, was sent to me to be destroyed. At nine o'clock in the evening, *two grains* of strychnia made into a pill with linseed meal and water, and loosely wrapped in a morsel of thin paper, was forced down the dog's throat, followed by a little water; the animal was then turned loose. The owner thinking he heard it, went out to look for it about two o'clock in the morning; he found it in its kennel, to all appearance suffering a good deal, gasping, &c., and it died shortly after. Here then is an

interval of *five hours* from the administration of the strychnia until death took place, and the dog performed the journey of a mile to get home. The retarded action may possibly be accounted for by the strychnia having been made up with linseed meal, which, when once wet up into a mass, does not readily separate on the addition of more liquid. Strychnia was not used with any special object, but merely from the owner of the dog having suggested it, supposing it would be a speedy means of destroying it.

In the following case, sent to me by Mr. Ashwin, of Abergavenny, *four grains* of strychnia produced in a dog no symptoms for the long period of *eleven hours*.

3. "A veterinary surgeon, in this town, had occasion to destroy a vicious, blind, and therefore useless pointer dog, for which purpose he gave it *four grains* of strychnia, laid between a *fold* of raw meat; this the dog swallowed readily, and was watched for *more than an hour*, but no effect of the poison was produced. The animal was occasionally seen afterwards during the day, and again the next morning two or three times, and upon the last occasion he seemed as well as before the poison was given. The person who gave the strychnia had occasion to leave the stable; he was not away more than two minutes, but upon his return found the dog dead. This was *eleven hours* after the animal had taken the poison. Thus it seems to me, that until the food was digested the strychnia remained innocuous; but no sooner had it come in contact with the stomach than its effects were actively shown. It would appear, therefore, that before we say poison like strychnia cannot have been taken, because so long an interval has elapsed before symptoms, the mode and circumstances of administration should be inquired into."

Mr. John Ewens, of Milton Abbas, communicates the following:

4. A patient, an intimate friend, having a greyhound useless from age, wished it to be killed, and requested me to poison it. Accordingly, about ten minutes before nine o'clock, in the evening, I administered *two and a half* grains of strychnia (as obtained from my London druggists), mixed with a little lard on bread. Not until ten minutes before ten o'clock, did the dog manifest any unusual symptoms, except that of a morbid appetite; but nothing else was noticed. At the end of an hour, I fancied that the dog seemed slightly affected with spasm, but this passed off (and was not well marked). At five minutes past ten o'clock, the first *violent* spasm occurred, and with slight intermissions increased in severity for about twenty minutes, the dog meanwhile lying on the ground on its side, and in the position so graphically described by various witnesses at the trial, breathing with the thorax only, abdominal respiration entirely absent, voiding its urine, &c., &c. At the end of twenty minutes, the dog got up and walked about, but seemed in a state of great agitation. Nevertheless, the dog appeared better, and I thought, perhaps, it might have recovered, but my friend not liking to see the poor animal suffer, despatched it at the end of about forty minutes from the commencement of the spasms. It appears to me, that this case bears strongly in favour of the presumption that Cook was poisoned by strychnia, for if a comparison were drawn, it would be found that the time between the administration in this case and the occurrence of the symptoms agrees exactly with the difference in the time of the supposed administration in Cook's case, and the occurrence of the symptoms; and this appears to me important, as a very strong point in the defence was the assertion of several surgeons, that the

length of time elapsing, was strong presumptive evidence that no strychnia was administered.

Mr. Ewens forwarded to me some of the strychnia employed in the experiment: it was mixed with brucia.

I am indebted to Mr. Thompson for the following fact:

Four grains of pure strychnia, obtained from Mr. Morson, were given to a dog suffering from mange, but except a prolonged sleep, it produced no effect. Two days afterwards, Mr. Thompson gave the animal *ten grains* of crystals of sulphate of strychnia. The animal died in eight and a half minutes. Four months afterwards, Mr. Thompson examined the remains of the dog by the acetic acid and charcoal process, using alcohol for a solvent of strychnia: and by the bitter taste and the bichromate of potash test, he detected strychnia. Out of the ten grains administered (the animal dying in the very short period of eight and a half minutes), Mr. Thompson thinks he recovered half a grain. This confirms the observations made at p. 124 and 131. One twentieth part of the dose given is calculated to have been recovered. The remaining nineteen twentieths, from some cause or other, had disappeared from the body.

Some professional correspondents have suggested, that in order to justify the very positive opinions given by the chemists for the defence at the trial, they should have experimented on animals which had been previously dosed with tartar emetic, and not have given such large doses of strychnia alone to animals which had not been subjected to a preparatory course of antimonial poisoning. The results might have been different. Others have suggested that, in Cook's case, the strychnia may have been mixed with morphia in the pills in sufficient quantity to account for the protraction of the first appearance of symptoms. It is quite intelligible, that a man who would coolly prepare his victim with antimony, might combine morphia with strychnia, so as to mask in some measure the ordinary effects of the poison; but it has already been established, by a reference to other cases, that the history of the case of Cook, taken as a whole, is in accordance with the action of strychnia.

Mr. Clayton, of Birmingham, from a desire to ascertain the truth, undertook some experiments in reference to the influence of antimony and morphia on the action of strychnia. He says—

“We took three strong dogs, two black and tan terriers, the third a half-bred Scotch: to the latter, I gave, about eleven o'clock, one grain of tartar emetic, which did not make him sick; we then, about half-past three o'clock, gave to it and one of the others, a grain of strychnia each, made up into a pill with jam; to the third, we administered one grain of strychnia and one grain of morphia, similarly prepared. The result was as follows: in eight minutes, the dog to which antimony had been previously administered became uneasy, and whined; it then had twitchings in the hind legs, followed by panting respiration; it then fell on its side, and after seven or eight convulsions died, in sixteen minutes from the administration of the strychnia. In *twenty-five minutes*, the dog to which strychnia *alone* had been administered was seized; this, like the last, became uneasy, and had twitchings of the *hind* legs; it then straddled with them a good deal, but still could walk; next, all four legs were drawn together, so as almost to touch; it then fell heavily on its side in a

strong tetanic convulsion, which lasted a couple of minutes; the limbs were relaxed, and upon rubbing its neck it seemed relieved and raised itself up on its *fore* legs, the *hind* ones appeared unable to support its body; it was then seized with another convulsion, and these continued following each other to more than a dozen in number, but getting feebler towards the last; after each convulsion, the breathing was extremely panting, and the action of the heart rapid and irregular; this dog died, partly from exhaustion and partly from asphyxia, one hour and five minutes from the administration of the poison.

"The last that was seized was the one to which the morphia and strychnia had been administered; it did not manifest any premonitory signs until thirty-five minutes had elapsed; they were then much slighter than were perceived in the others; its powers of volition being greater; its respiration being more hurried in proportion to the twitchings of the limbs; whilst walking somewhat unsteadily about four minutes after the uneasiness commenced, it fell *forwards* on its head, in a tetanic convulsion, this lasted rather more than a minute, and was almost without an intermission followed by a second; another short period of comparative repose and its life was terminated (five minutes after the first), by a third convulsion, it died more quietly than the other two, and did not like them micturate with each convulsion; its death took place forty-five minutes from the administration of the strychnia and morphia, so that it would appear that the antimony had promoted the action of the poison, and killed the quickest, whilst morphia delayed the accession of them, and rendered the premonitory symptoms longer and slighter, but killed with much fewer convulsions. It is therefore unfair to compare Cook's case, in which other remedies had been taken, with cases in which strychnia alone has been given. In none of these cases was there lock-jaw; *all were relieved by rubbing the neck*; in none did it bring on convulsions; touching them, however, with the front of a finger when the fit was either coming on or going off produced the same effect as a galvanic shock, this did not occur during the period of repose; all were very rigid during the fit, relaxed during the interval, and at death; the rigidity, however, returned in all, a quarter of an hour after death, and a post-mortem was made fifteen hours afterwards; they were all more rigid than is normally the case, but the one to which morphia had been given,—much more than the others; the muscles of the loins and thighs were as hard as iron, but the face was expressive of repose. The viscera were observed *in situ*, all of them were opened together, and scarcely a variation could be observed; the right auricles were full and congested, the right ventricle uncontracted, the left ventricle contracted, but the auricle on that side partially full; the blood fluid in all, but less so in the one to which strychnia only had been given; the lungs congested, more particularly the left, the vessels being fuller in that lung; the livers congested; the intestines natural:—in the one to which strychnia only had been given the bladder was empty; in the others it was full; nothing particular about any of the kidneys. The brain and spines not examined."

It is to be observed, that in the case of Mr. Cook, there is no evidence that a dose of tartar emetic was given to him shortly before the administration of the strychnia. His system was no doubt depressed by the repeated doses which he had taken during his illness, and this may probably have rendered him less able to resist the violent effects produced by the strychnia when the action of the poison had once commenced.

ON THE DETECTION OF STRYCHNIA IN CASES OF POISONING  
BY THIS SUBSTANCE.

Letters received from thirteen professional gentlemen, in different parts of the kingdom, refer to this subject. Most of these bear upon the allegation that the detection of strychnia is interfered with by the presence of tartar emetic, tartaric acid, common salt, the nitrates, cod-liver oil, and other substances. They show a wholesome feeling in some members of the profession—a desire to arrive at the truth, and not to conceal or palliate the crime of murder, although committed by one who has been described as an “injured professional brother.” One of these writers suggests, that the witnesses for the defence, who swore that strychnia always was and always must be detected, when it destroyed life, should have been asked—“What would be the chance of discovering a small dose of strychnia, if administered after the patient had been well dosed and depressed by the action of tartarized antimony? I believe that Cook was so thoroughly knocked down by antimony that the *very smallest* dose of poison only was required to kill him, and consequently the possibility of finding the poison would be one hundred to one.”

Had the question suggested been put, it is highly probable the witnesses would have replied, that the previous dosing with tartar emetic would have made no difference. As it was, they tried the effects only of relatively large doses on animals, and then gave on oath speculative opinions as to what would be the results physiologically and chemically, when a minimum dose destroyed the life of a human being. In the Appendix (p. 138), I have related a case, in which a child was poisoned in four hours by the sixteenth part of a grain of strychnia. If such a case as this had been put hypothetically at the trial, it is by no means improbable the witnesses would have sworn that the strychnia must be detected in the body or death could not have taken place from it. This is the opinion to which they are, in fact, pledged in all future cases of poisoning by strychnia, whatever may be the dose, and whatever the circumstances. They virtually deny by their evidence the possibility of an exception.

It is to be borne in mind, however, that the same witnesses had stated on oath that the death of Cook was not consistent with the effects of strychnia, on account of an hour and a quarter having elapsed before symptoms appeared, the sudden invasion of these symptoms, the action of vomiting, swallowing, and beating the bed, the tolerance of rubbing of the skin, and lastly, in the dead body, the emptiness of the heart—differences which are proved by facts to have no real existence. Their opinion regarding the uniform detection of the poison in a clear and unequivocal manner, is inconsistent with what is at present known respecting poisoning in the human subject; and no doubt cases will hereafter occur which, unless they happen to fall into the hands of sanguine chemists, will present just the same difficulty in this respect as the case of Mr. Cook. In the mean time, the death of Mrs. King, in Jamaica, on the 2d of September, 1856, from a dose of strychnia administered by mistake, furnishes a comment upon the chemical evidence for the defence in the case of Palmer.

It appears that among some medicines sent to Newcastle from the medical dépôt at Kingston, were two bottles containing respectively strychnia and oxide of bismuth.

Both bottles were labelled "bismuth," and were sent to Newcastle thus labelled. Mrs. King, the wife of Lieut.-Colonel King, of the 36th regiment, arrived there slightly indisposed, and the medical officer, Dr. Jopp, prescribed for her three powders, each consisting of five grains of oxide of bismuth. The hospital-serjeant, Price, who received the prescription, seeing two bottles with similar labels, put the contents of the two into one, and unfortunately placed the strychnia in the bottle containing the oxide of bismuth. Hence five grains of strychnia were weighed out in place of oxide of bismuth. The medicine when taken produced the usual symptoms of tetanus, and proved fatal very speedily. When Dr. Jopp was informed of this, he felt so confident that the death of this lady was a coincidence, and that the medicine which he had prescribed could not have produced it, that he offered to take one of the remaining powders himself. Colonel King would not allow him to incur this risk, observing that the loss of one life was sufficient. A powder was then given to a large and powerful dog, which died in a short time with the symptoms of tetanus. The remaining powder, with the contents of the stomach of deceased, and the bottle from which the powders had been weighed, were sent to Mr. Grant, analytical chemist, for examination. The analysis was made by him, assisted by Mr. Burgen and Mr. Amorey, with the following results: The remaining powder consisted of pure strychnia; the bottle contained a mixture in the proportions of sixteen parts of strychnia to fourteen parts of oxide of bismuth, while in the *contents of the stomach of deceased no traces whatever of strychnia were found*. The jury returned a verdict of manslaughter against Dr. Mosse, of the medical dépôt at Kingston, who sent the two bottles, and against Dr. Jopp, who prescribed the medicine.

*The Colour-tests.*—In commenting on the colour-tests for strychnia, at page 90 of the text, I have omitted to quote the opinion of an able French chemist, M. Chevallier, the editor of the 'Journal de Chimie Médicale.' It fully confirms the views of Orfila.

In describing the substitution of ferricyanide of potassium for bichromate of potash in testing for strychnia as recommended by Dr. Davy, Chevallier says :

"Nous pensons, toutefois, qu'on ne saurait se montrer trop réservé dans les conséquences rigoureuses qu'on en peut déduire. En médecine légale ou la deposition du chimiste a toujours *une gravité* qu'elle n'a pas dans les cas ordinaires, *les phénomènes de coloration* dont il vient d'être parlé seraient certainement *insuffisants pour permettre de se prononcer avec certitude sur l'existence d'un corps tel que la strychnine*, et il serait facheux qu'on s'y attachât d'une manière trop exclusive et trop absolue. La réaction indiquée par M. Davy doit être considérée comme un moyen auxiliaire très utile et très précieux *a l'appui des autres caractères* signalés pour la strychnine, mais *nullement* comme un *indice absolu et certain de la présence de cet alcaloïde.*" ('Journal de Chimie Médicale de Pharmacie et de Toxicologie,' 1853, p. 732.)

I have been recently informed by Dr. Hofmann, that aniline, a product of the decomposition of organic substances so widely different from each other as indigo and benzole, produces no colour with sulphuric acid until bichromate of potash is added. A splendid violet colour is then produced. The artificial production of this

colour is the subject of a patent. Aniline appears as a very frequent product of the decomposition of nitrogenized substances; and probably no organic compound has been formed in so many different ways as this. Aniline may be very easily distinguished from strychnia and strychnia from aniline, when there is sufficient of either for the experiment: but the above fact should teach caution as to an absolute reliance upon the production of colours in organic extracts from the admixture of sulphuric acid and the bichromate of potash.

*Alleged decomposition of Strychnia.*

At page 123, some remarks have been made on the probable decomposition and loss of a portion of strychnia in cases of poisoning. In Dr. Geoghegan's case (referred to at page 146), it is worthy of observation that, by the most careful research by Stas's process, not the slightest trace of strychnia was detected in thirty ounces of urine passed by the patient from the fifth to the thirty-first hour after symptoms had commenced. What had become of the strychnia? If it remained in the body the man ought not to have recovered; and if it passed out by the urine *unchanged*, as it was alleged in the defence, it ought to have been found by the chemical process employed.

In a paper published in the 'New Orleans Medical Gazette,' for September, 1856, (p. 387,) Dr. Crawcour, Professor of Chemistry and Medical Jurisprudence at New Orleans, makes the following observations on the question at issue:

"To assert that the minutest quantity of any poison can be detected *in* the human body, because an infinitesimal quantity can be detected *out* of it, is to assert a dogma that yet remains to be proved." After stating that analogy is strongly in favour of the view that strychnia like caffeine, thein and quinine, does undergo changes in the organism, he contends that "we can only arrive at the truth by administering to animals the *minutest doses* of any organic poisons that will cause death, or even such as are insufficient to destroy, and then searching in all the tissues for the poison." \* \* \* In reference to the trial of Palmer, "Dr. Taylor mentioned that he had given to a rabbit two grains of strychnia, and had detected the poison with ease: to another he had given one grain, and could only perceive a bitter taste: in a third, killed by half a grain, no trace of the poison existed. It would seem, therefore, that half a grain is the quantity capable of assimilation in a rabbit. In order to examine for myself, I gave to a rabbit *half a grain of strychnia*, and to render it more easy of absorption, I administered it in solution. The rabbit died *in half an hour*. *The next day I made a rigid and searching analysis, but could find no trace of strychnia.*"

This is the testimony of a scientific observer, living at a distance of some thousands of miles, and having no interest to support the view taken either for the prosecution or defence at the trial of Palmer! Such independent testimony as this, taken together with other facts of a similar kind recorded in this paper, shows that the main point in the defence of WILLIAM PALMER turned upon as gross and dangerous a fallacy as ever was propounded in an English Court of law!







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