

Supplement to Some observations on the muscular contractions which occasionally happen after death from cholera: containing a note of a case of apoplexy, after which similar movements were remarked / [William Frederick Barlow].

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SUPPLEMENT

TO SOME

OBSERVATIONS ON THE MUSCULAR CONTRACTIONS

WHICH OCCASIONALLY HAPPEN AFTER

DEATH FROM CHOLERA:

CONTAINING A NOTE OF A

CASE OF APOPLEXY, AFTER WHICH SIMILAR MOVEMENTS
WERE REMARKED.

BY

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THE more attentively the subject of post mortem contractions is considered the more worthy of study will it appear; and it is really a fit matter for astonishment that it should so slowly have attracted the thoughts of inquirers. Like every other topic actually submitted to contemplation, it amplifies as one views it; what once seemed *peculiarities* take another shape, and the air of strangeness which it wore at first sight fades gradually away. Phenomena gather more thickly, are more illustrative than might be imagined. If observers would but take the trouble of fairly and intelligibly recording all the facts which touch upon this interesting question, we should soon obtain a history which would do much more than repay perusal; but some have thrown away the opportunities which have occurred to them of illustrating the inquiry, or have so dimly, so superficially observed, that their remarks are of but trifling value.

In the present state of the question facts are yet acceptable. I have been indebted to Mr. Kesteven for the following particulars:—"In 1832 I attended a gentleman, aged 42 years, who died of cholera in fourteen hours. After death the muscles of the lower jaw continued to act for *two* hours: the jaw was not moved as in speaking, but the mouth simply opened and closed at regular intervals, which gradually lengthened. In the same body the muscles of the abdomen were observed to have a tremulous movement. The muscles of the thighs also, more particularly the sartorius, were in action; they contracted in spasmodic twitches, but not sufficiently strongly to raise the limbs, though these were slightly moved."

This is clearly to be placed amongst the most striking class of cases. The movements of the jaw remind one of what is sometimes witnessed in the heads of decapitated animals. The time of their continuance deserves particular notice. I believe that contractions of this kind endure seldom so long as two hours; but one ought to speak

with deference on this point, because future inquiry may find them to be renewed after a longer period of cessation than we are now acquainted with. The form of the motions was peculiar; and they appear to have been rhythmical, or almost so, and unlike any voluntary action. I have noticed *rhythmical* movements in insects, from which I had removed the heads, that I think should be classed with such as these. The intervals between the movements lengthened: this circumstance probably indicated, and was caused by, diminished irritability of the muscles. The tremor of the abdominal muscles observed here has not been, so far as I know, nearly so frequently seen as that of the muscles of the legs, arms, chest, and face; for what reason I cannot tell.

Dr. Risdon Bennett some time ago informed me of a case in which the respiratory muscles seem to have been spasmodically affected after indubitable dissolution; and I refer to it on the ground that contractions of those muscles would be especially likely to delude some persons as to the fact of dissolution being absolutely real.

Amongst my own too many omissions I regret to have made no special observations as to the time of occurrence, degree, and duration of rigor mortis, in those cases wherein post-mortem movements or quiverings were seen: these points require investigation.

Inquiry as to the event of post-mortem contractions should obviously not be confined to cholera; so to restrict it would be at once to narrow and mystify the question. We need a knowledge of *all* the circumstances under which they occur. Some writers would imply, by the way wherein they mention them, that they have only been noticed after Asiatic cholera; but this is a mistake, and needs well pointing out, inasmuch as it might lead a person to suppose that the poison of cholera of this type, whatever it may be, or the peculiar state of blood occasioned by it, is necessary to their production.

It may reasonably enough be made a question whether similar movements do not ever happen after English or common cholera.

A fatal case of this form of disease occurred this autumn in the Westminster Hospital. It was extremely severe; the temperature was strikingly diminished; and, but for the *bilious* dejections, it might have been called Asiatic, if we may still apply that epithet to a malady, *now*, most unhappily, European also. The patient, a man æt. 40, died, was laid out in the usual way, the arms being placed straight by the side. In about an hour the arms were found bent at right angles, one hand being placed upon the chest, and there was complete rigor mortis.*

There are two points of interest here: first, the early setting in of rigor mortis; secondly, the remarkable change in the position of the arms. What was the latter owing to? The most probable explanation that can be offered is, that first they were moved by muscular contraction, such as depends upon irritability, and then were fixed in their changed position by the quick supervening of rigor mortis. If this explanation be rejected, it must be attempted to account for the altered posture by referring it to the rigidity of death. Now there is nothing in the present state of our knowledge, so far as my information goes, which would justify us in so referring them. Nysten went the length of stating that "the position of the limbs always remained unchanged" by rigor mortis; and who has treated of this phenomenon more forcibly, more delicately, more beautifully than he? His authority may be cited against such a notion. Nor is there anything in the opinions of M. Sommer, as stated by Professor Müller, which would warrant the conclusion that *so marked* a flexion as that noticed in the case before us could be satisfactorily explained in the manner mentioned. Let me, availing myself of Dr. Baly's translation, lay the passage alluded to before the reader. M. Sommer states "that a *slight* motion takes place when the muscles become rigid. He affirms that Nysten was incorrect in stating that the position of the limbs always remains unchanged.

The lower jaw, he says, though it be separated from the upper jaw at the time of death, becomes afterwards firmly drawn up towards it. The extremities also become more strongly flexed: thus the thumb is drawn towards the palm, and even the fore-arm is a *little* bent on the arm."*

I have ventured to put two words in italics; and I think that, admitting M. Sommer to be right, and Nysten to be in error as to the point disputed, we cannot properly conclude that the flexion in the example stated was, looking well to the *degree* of it, produced by the rigidity of death. If it really were so, it is quite clear that so extreme an effect of the phenomenon is unusual.† I should have dwelt thus far upon the change of posture were it only for its physiological import; but it has also a most practical relation to medical and medico-legal inquiry; for it is very plain that such alteration of position might lead to the conclusion that a person had been pronounced dead before he really was so, to the prejudice perhaps of his medical attendant, and the discomfort of his friends, who might even imagine that some chance of saving life had, owing to ignorance or inattention, been thrown away.

It is to be regretted that the body in the case narrated was left unwatched between the time of death and that of the coming on of rigor, for it would be important to be able *certainly* to state that muscular contractions had occurred. As it is, I can merely affirm my belief that they most probably did happen, and that it will very likely be one day established that such contractions may occur after rapidly fatal English, as well as after rapidly fatal Asiatic or epidemic cholera, and that no distinction between these affections or forms of affection can be based rightly upon them.

It is to be supposed, since cases differ greatly, and also the subjects of them, that post-mortem muscular contractions may be more common in some epidemics than in others: and I have been informed that they were not so frequent in Paris during the late visitation as in a former one.

In relation to post-mortem muscular

* This case happened during my absence from town. I owe the account of it to Mr. Kidd, and notes taken by Mr. Morris.

* See Elements of Physiology, p. 890.

† I have made a few remarks on the alteration of the position of the dead in a former contribution on the Condition of the Body after Death by Cholera. See MEDICAL GAZETTE.

contractions in general there is nothing of higher importance to determine than the various circumstances under which they may happen. When are they most likely to be observed? When most likely to be observed most strikingly? What does pathology, what does experimental physiology, say respecting them? How is it that they were only first observed since cholera in a malignant form was known to us?

Dr. Dowler, whom I have referred to in previous remarks, on account of the importance of the *facts* he has narrated, has put on record a well-marked case wherein these contractions happened after yellow fever—a disease, it will be remembered, in which he has shown repeatedly how readily muscles may be excited to contraction after life is over by means of percussion.

What, more especially, are the forms of *sudden* or *rapid* death, for I suppose they are quite limited to such dissolutions, in which these contractions, if seen at all, would probably be found most violent and most protracted?

I have referred already to two instances of muscular quivering which were remarked on post-mortem examination: one of them was a case of tetanus. In this affection the disposition to spasm is, despite exhaustion, extreme unto the end, and there are manifestations of irritability when the general powers are too obviously declining.

I find the following passage in a case of disease of the spinal cord described by the distinguished Bellingeri:—"The spasms disappeared for a short time, and returned with greater severity, so that death appeared to have been produced by asphyxia, owing to a spasmodic action of the glottis. Immediately after death the left arm was drawn spontaneously backward."

I do not remember to have read any case of post-mortem contractions in the writings of Sir Chas. Bell, Dr. Marshall Hall, Dr. Abercrombie, and others: nor in those of some celebrated French authorities on the nervous system. Dr. Marshall Hall, as I can state from conversation with him, has never yet seen any after disease. The most distinguished surgical authorities on injuries of the brain and spinal cord make no allusion whatever to the matter. And yet some diseases and injuries of the nervous system occasionally destroy with an exceeding swiftness; the muscular system

is well, and sometimes even extraordinarily nourished at the time they surprise; the muscles must be not rarely extremely irritable at the moment of death; and there is, all things considered, a condition of the body wherein the occurrence of such movements might surely not unreasonably be expected; neither might they, one would think, be absurdly looked for after some abrupt terminations of heart affections.

It was not, however, until a short time ago that, with the exception of what happened in consequence of cholera, I observed any movements whatever after death, though I am no stranger to the aspect of the body on sudden dissolution taking place. I allude to a fatal case of Apoplexy, and will now endeavour to describe briefly some phenomena which succeeded to the last expiration.

John Reid, a well-developed man, ætat. 41, was admitted, one day last summer, at 1 o'clock A.M., into the Westminster Hospital, under the care of Dr. Basham. He had been apparently struck by apoplexy, was completely insensible, breathed very stertorously, the circulation was languid, and he was pale and cold. The pupils, unlike what generally happens in the apoplectic, were as narrowly contracted as I have ever seen them, either in sleep or poisoning by opium. He could be made to swallow with great difficulty. Some wine was, however, administered, and in a few hours he rallied considerably, at least so far as organic life was concerned; his breathing was less embarrassed, although still alarmingly impeded; his pulse became more vigorous and rapid, and there was a most palpable increase of animal heat, which I now regret was not measured by the thermometer. Towards evening he became obviously worse, and died at a quarter past 7 o'clock, no convulsions or spasm of the face or extremities having been noted previous to his decease.

Scarcely had he died, when the nurse who had attended on him hurried to me, and said that he was moving much as some of the patients did after death by cholera. I of course went directly to see the corpse, and found it was true enough. My attention was first attracted by motions in the feet. The toes were flexed; sometimes altogether, sometimes two, or one only, were influenced, and the contractions varied greatly

in force. The whole foot was distinctly moved several times, but not with any great degree of violence. The tibialis anticus was in a frequent flickering action. The whole of the muscle was not affected, at least not visibly. A narrow portion only, running its whole length, was seen to act; but so fleeting was the motion, that unless the eye was fixed upon the muscle at the very time of contraction it would have eluded notice. The motions of the toes could be much more easily surveyed. They now affected one foot, now both feet simultaneously. The gastrocnemii muscles were once or twice slightly influenced. The hands were slightly moved twice or thrice, and the fingers flexed several times. The motions would pause for a while, and then be renewed, coming and going just as I have seen them after fatal cholera. I could not *excite* discernible motions, either by flexing the limbs, or striking the muscles or pricking them. There were no motions of the face, thorax, or abdomen. The contractions were timed, and found to last *three-quarters of an hour*. They could not have been distinguished in any way from those which have been noted subsequently to cholera; and if I have seen some evidently more marked and striking after that disease than these were, I have likewise noticed others which were infinitely less so.

The surface felt extremely warm. I placed the bulb of a thermometer in the axilla *half an hour* after death, and ascertained a temperature of 104° . The body had been, judging by the touch, of an unusual heat during lifetime. I do not, therefore, mean to imply that there was any *rise* of heat. I do not think that there was, though it is impossible to affirm this positively.

Bodies should be watched closely and patiently after all kinds of sudden and rapid dissolution, by any one desirous of witnessing such motory phenomena as those described: experience only can determine how often search will be successful. But it must be remembered that the *most rapid* deaths, some through violence, and more in the course of nature, occur month after month, day after day; the subjects of them must often be observed more or less closely, and for a longer or shorter time after dissolution, by friends, or others with them when they die, and yet no motions are seen in such cases at all comparable

to those which happened in this apoplexy, though there may be occasionally a faint muscular quivering or two observed, enough to attract a passing notice. Only let us imagine the vast numbers who annually perish in hospitals, infirmaries, union houses, and private dwellings, without any such remarkable movements being seen. Still, in most new inquiries, one is supplied with more abundant instances than were hoped for. This is a consequence of research. Look at the many cases of reflex action in paralytic limbs which have now been seen at one time or other. Most of them would certainly have been passed over, but for special questioning.

We have long heard of post-mortem movements happening in India. Some formerly have either treated them as fables, or not troubled themselves about them because of their occurring so far away. So with many the cholera excites no interest now. They say it is in Malta, and elsewhere, but not here; that the topic is not popular, and they will think no more of it till the disease return. Should it again invade, there will once more appear a myriad of contributions, and the columns of journals, medical and general, will be forthwith crowded to inconvenient success.

It can with difficulty be supposed that the case of apoplexy which I have just sketched is the *only* instance of that affection in which these contractions have *taken place*, though it may be the only one in which they have been *seen* to any remarkable extent; and it may be conjectured that other cases of sudden or rapid destruction caused by injuries or diseases of the nervous system, or other injuries or maladies, or, perchance, by the speedy course of poisons, will, sooner or later, be found to exemplify them. It remains for future inquirers to treat the whole subject of them in its length and breadth; to note the species of death in which their supervention will be most probable; to mark accurately their periods of beginning, declining, ending; their course, their form, and what may best provoke them; to trace minutely every circumstance of interest, and to show with all clearness, fulness, and judgment, their every physiological and pathological bearing. What I have written is but a fragment.

Let me now subjoin a short state-

ment of the principal pathological changes observed after death in the preceding instance.

There was no peculiarity in the external appearance of the body, which was examined *eighteen* hours after death.

The limbs were by no means unusually rigid.

The vessels of the brain were more congested than usual, and a flattening of the convolutions was observed.

On opening the left lateral ventricle it was found completely filled with coagulated blood; the septum was broken down, and some bloody fluid, with scarcely any coagula, was observed in the fellow cavity. Some blood, too, had made its way into the fourth ventricle. The hæmorrhage seemed to have proceeded from the corpus striatum, thalamus opticus, and almost the entire portion of brain which formed the floor of the left lateral ventricle. This portion was soft, irregular, shreddy, and the blood which covered it was mixed here and there with diffuent cerebral matter. Numerous bloody points remained diffused everywhere, after water had been gently but freely poured over it. There was no marked degree of softening in any place, if we except that part of the brain which lay just immediately beneath what appeared the direct seat of the apoplexy.

The arteries at the base of the organ presented unmistakeable and pretty general signs of fatty degeneration. The heart, which was, I think, to some degree enlarged, showed a marked rigor mortis, such as formerly led to the use of the erroneous term "concentric hypertrophy." The aorta was thickly studded with atheromatous or fatty deposit, and there was a considerable layer of fat upon the surface of the right ventricle. The kidneys, which lay imbedded in most abundant fat, were atrophied, and far advanced in granular degeneration. Some urine was in the bladder, which was highly albuminous.

Thinking it most likely that the muscular fibres of the heart would be to some extent in a state of fatty degeneration, though, looking to the rigid state of the organ, not far gone therein, I requested Dr. Quain to examine it microscopically; but, through accident, the heart did not reach him in a state sufficiently fresh to admit of that satisfactory scrutiny which was desirable.

Dr. Quain observes, in a letter with

which he favoured me,—“There is abundant evidence of granular fatty matter in the *muscular fibre*. This appearance exists throughout several parts of the heart that I have examined; but the granules are very small. The degeneration, therefore, though extensive, is not greatly advanced. It is possible that the presence of some of this matter is due to decomposition; still I feel quite satisfied, from the general appearance of the heart, that fat would have been found if the heart had been examined when fresh.”*

Examinations of cases of this kind cannot, of course, be satisfactory without the microscope be used. The pathology of fatty degeneration is only to a slight extent explorable by the unaided eye. I can hardly doubt that, had the small blood-vessels of the brain been examined, we should have discovered in such degeneration of them the precursor and cause of the apoplexy. I rest this observation on a communication of Mr. Paget to the Abernethian Society, “On Fatty Degeneration of the Small Blood-vessels of the Brain, and its relation to Apoplexy,”†—a contribution to pathology of the highest value.

It would have been necessary to have apologised for making such special reference to the fatty degeneration of the heart which was observed, had not the subject of that common change the widest possible relation to the whole question of muscular irritability, whenever, however manifested.

The relation of such degeneration to the irritability of the heart is every now and then emphatically exemplified by the action of the organ suddenly failing, and as suddenly producing death. The proper stimulus is not withdrawn, but the damaged fibre fails to respond: the stimulus cannot act on it, as galva-

* The just published volume of the Royal Medico-Chirurgical Society contains a most admirable contribution, by Dr. Quain, on Fatty Diseases of the Heart.

† Since published in the MEDICAL GAZETTE. Fatty degeneration will, it may be fairly anticipated, be the means of explaining some forms of paralysis, both cerebral and spinal, some diseases of the special senses, and some affections of the mind, especially in aged people, whereof the immediate cause is at present hidden. In a case of nearly complete blindness (the subject of which was a patient of Mr. Holt's) the optic nerves were found after death to have most extensively undergone this form of degeneration. They were first examined by Mr. Holthouse, and afterwards by Mr Dalrymple, who were both quite satisfied on this point. I had the opportunity of examining them.

nism cannot influence an irritable muscle. It must be observed that a heart wherein fat has taken, to a serious degree, the place of the proper irritable tissue, may be equal to ordinary, though not to extraordinary, occasions. Shock of mind or body, intense emotions, whether of joy or sorrow, syncope, and all accidents and diseases which kill quickly by asphyxia, must obviously be far more perilous to those whose hearts are thus affected than to others; means of recovery would be lost in them which might succeed in others; the suspended animation might be death in them which would not in others. And every cardiac fibre is impaired in use just in proportion as it is in texture, whilst the entire destruction of the irritability of a single one is a type of what may eventually happen on a large, and perhaps fatal, scale.

In some cases of the change, the voluntary muscles, from having extensively suffered in like manner, would probably be found less susceptible of galvanism than they commonly are. It is not pretended that this would have been discovered, had a trial been instituted, in the instance I have mentioned, for the degeneration of the heart was not in any sense nearly so marked as it is oftentimes; and the contractions of the muscles, seen after dissolution, lead to the conclusion that their fibres were entirely or almost sound.

Nothing can be plainer than that this degeneration may greatly modify, and, at times, prevent every kind of muscular action. A cause of cramp, it may be assumed, would not affect an excessively fatty muscle as it would another; nor would galvanism, nor strychnia, nor heat, nor cold, nor emotion, nor the will, nor any other sort of known excitant; neither, *cæteris paribus*, could we so reasonably look for post-mortem contractions in such a muscle as in one unimpaired. And we may safely believe that, amongst the causes which may prevent, or aid in preventing, the occurrence of post-mortem contractions in some cases of sudden death by apoplexy, the change in question merits distinct notice.

Certainly such contractions could not occur in the altered parts, no matter what the circumstances of death, in such a case as that described by Vicq d'Azyr, the subject of which had almost all the muscles of the leg converted into

fat or cellular tissue, and "was obliged in the latter part of his life to walk with crutches."* We must not, of course, ever confound the fat deposited upon, with that which takes the place of, a structure—for there is a wide difference between addition and destruction—nor suppose a part to be necessarily degenerated because largely and perfectly surrounded by it. "Fat," says Mr. Paget, "accumulated in tissue round a part is a very different—probably an essentially different—thing from fat within it: the one is compatible with perfect strength; the other is always a sign of loss of power. In the muscles of some fish—the eel especially—it is hard to get a clear sight of the fibres, the oily matter round them is so abundant; but the fibres are peculiarly strong, and, in their own texture, make a striking contrast with the fibres of a degenerate muscle in which the fat is in great part within."†

It might have been laid down from ordinary reasoning that the structural state of muscles has much to do with the post-mortem contractions which happen in them, and that it could not be otherwise; nor need we wonder that males should manifest them more frequently than females, and the strongest males most of all. It may be, occasionally, that the reason of some muscles of the same subject contracting to the exclusion of others is, that they are more irritable in consequence of being better nourished.

We need not be too surprised at the after death contractions of cholera being so partial and disuniform: for only let us fancy the very varying states of the muscular fibre existing in different instances at the time of dissolution; the unlike degrees in which this fibre has been exhausted in the course of the disease; the diversified states of its nutrition; and the dissimilar condition of the blood in its vessels; and we shall, to rest here, see no just cause for extraordinary amazement. The physical state of man—to say nothing of his mental—is never, perhaps, *exactly* the same throughout any two given minutes of an existence singularly impressible, and

* See Corvisart's observations "On the Conversion of the Muscular Tissue of the Heart into a Fatty Substance." *Diseases of the Heart*, p. 165.

† See Lectures on Nutrition, Hypertrophy, and Atrophy, *MED. GAZ.*, 1847, p. 150.

for ever fluctuating. Never, as Bichât insisted long ago, shall we understand life's mechanism in the sense that we comprehend the workings of instruments of our own invention; but we shall be perplexed always as to many matters, because we have not *all* the data which their solution requires, and, from the very nature of the problems, cannot, nor even reasonably hope to, have.

These contractions come and go much as cramps do during life; a renewal of irritability appears indispensable to the continuance of both sets of actions, and its temporary exhaustion accounts, apparently, for their so common intermittence. The varying periods at which they begin after dissolution is, it may be imagined, in some way connected with unlike conditions of the fibres, at the moment of dying, as to the exhaustion or renovation of their power; and in the interval between the last expiration and their commencement it is not improbable that their force may be renewed: for there are facts to show that irritability may be restored or augmented, of course within limits, even after the circulation of the blood has ceased. The cause of these movements in the dead might in some instances, had it acted in the living, have provoked cramps. But one thing seems certain; they generally suffer most from cholera who have the most irritable and least easily exhaustible fibre at the time of its attack, the muscular energy heretofore so valuable to, and, perchance, by some men envied, them, becoming subservient to their exquisite agony, and rendering them liable, beyond all others, to those contractions which happen, sometimes, after the end of life.

It would, as I have hinted on a former occasion, be obviously of great interest to inquire into the condition of the muscles after death in cases of cholera by means of galvanism, for though the plan of percussion has been known to succeed in producing action, it would be absurd to use it for any precise or delicate purposes, in the place of an agent so subtle, so powerful, so sure, and effecting the mildest or most violent results in strictest obedience to the wishes of the experimenter. It would be desirable to know whether the muscles of those who manifest contractions

when life is over, respond to this stimulus with an unusual readiness, and whether there be any ascertainable difference of irritability in favour of those muscles, to take some given instance, wherein they most frequently and energetically operate. It would not be useless to contrast, moreover, the muscular state of those dying in the cold stage and those perishing subsequent to reaction. And the opportunity might be taken of determining the effect which violent and repeated galvanism *might* have, through its exhausting the irritability of the muscles, on the period of the approach of rigor mortis.

I know not whether any observations have been made, by the assistance of galvanism, on the irritability of the muscles during life in cholera. It would probably be found, judging both from the various conditions of the muscular system of the subjects affected by it, and from the great dissimilitude prevailing as to the amount of cramp, to be very different in different cases. The cause of the cramp may be in greater intensity in one case than another, and so irritate the muscles more, but the state of the fibres which manifest the phenomenon is, doubtless, dissimilar, and so extremely modifies and influences the action of that cause, that it would be preposterous to attempt to measure, as a general rule, the severity of the disorder by the degree of spasm. And, indeed, we know how many have sunk, unresistingly as it were, and as if overwhelmed by some mortal shock, without much suffering from muscular disorder. We must, if possible, so view the state of the muscles after death as connected with that which immediately preceded, as to make one continued history. The most violent cramps have affected the living where the most remarkable movements have stirred the dead; and I am not aware that muscular contractions have ever been noted, save in those who perished in the cold stage of cholera, the decline of which is the decline also of the spasmodic seizures.

I have said, that were galvanism employed to test the irritability of the living in this disease, that most likely the degree would be found very unequal. The amount of a property must, of course, not be judged of by its abnormal manifestation; the healthy who have no cramp, no convulsion, may obviously have more of that peculiar force, whereupon such

phenomena depend *immediately*, than those whom they affect. In the former there is nothing to provoke such actions.

On one occasion I passed a very slight galvanic current through the lower lip of a man who lay fast sinking from cholera. The muscular fibres responded readily. Upon that I tried the lowest effectual force, and then applied the same power to the lip of a friend of mine who was by. It had, as far as the experiment could decide, the like amount of irritability. My own lip, on being similarly galvanised, was found more irritable than that of either.

There is, I think, no question more intricate, delicate, and difficult of investigation, than that of muscular irritability; and it is partly made so by its very opposite states in such as really are, or are at least supposed to be, without traces of disease. Modified extremely by the processes of digestion, respiration, circulation, all the delicate and in great part inscrutable operations of nutrition, sex, age, occupation, fatigue, repose, and passions of the mind, is it wonderful that when we come to measure its relations to different individuals, and to the same individual at different times, that we should find our task one of great difficulty? And yet I doubt not that the subject of irritability would be found full of very many new applications by any able inquirer who would be prepared to contemplate the matter largely, and to make experiments with skill and patience.

I have, in the course of my observations, always spoken of the movements which took place after the cessation of the breathing and circulation, as occurring *after death*. Some, perhaps, following those who have written elaborately on what death is, may say that I have assumed the question. It was necessary to employ the word, not so much in a sense of refined philosophy, as in a wide and general meaning. I have used it, not only as people in general, but as the best writers on surgery and medicine are wont to employ it. I may be told that putrefaction has been pronounced as the only sure sign of death, but is not every one who dies, or almost every one, pronounced dead without such testimony? Would a physician, by the side of the dying, on being asked at some given moment if he were dead or no, say that he cannot tell, that he will

not be positive until some signs of decay are unmistakeably visible? No: he satisfies himself that the so-called "vital functions" are extinguished, and speaks accordingly. I happen to have lying before me Corvisart's work on diseases of the heart. I have made mention of death in the sense wherein he uses it in the following passage:—"I shall freely call the heart the main spring of the human machine; for let its action be suspended but for a moment, and there is apparent death; let it cease altogether, and there is real and sudden death."* Of course we must always have great care to distinguish between suspended animation and dissolution; and in this the stethoscope will be of importance sometimes, as has been strongly insisted on by M. Bouchut. But it will occasionally be impossible for a brief space to distinguish, let us be as cautious as we will; the real state will be proved by the issue. In such a case we must act as though the dissolution were apparent only, since the nature of that issue may turn upon our own efforts.†

Haller speaks of the *vis insita* of muscles being "proper to life and the first hours *after death*."

Dr. Dowler, in his observations on post-mortem muscular contractions, has drawn attention to a subject of very considerable moment; and I trust it may not be thought superfluous to allude to it. He says,—“Post-mortem contractility, in its legal applications, is not without interest, showing as it does the absurdity of some grave *judicial decisions*, based solely on muscular motion, which latter has been held to be a sufficient proof of life in a new-born child, enabling the husband to inherit the estate of his wife during his life-time.”

It appears to me that lawyers have been placed in difficulty on this point by medical witnesses, and that it cannot be disposed of in the summary style of the American physician. He affirms, speaking of a particular case, that

* He adds the following note:—"It will not, surely, be objected to me, that, in death, a capillary circulation still exists for some time; that the hair, the beard, &c., grow in a dead subject; I hope I shall not be thought ridiculous, if I say, I may be dead, although his beard sprout."

† Lord Bacon says, in his "Historia Vitæ et Mortis,"—"Ad resuscitandum eos, qui deliquia animi aut catalepses subitas patiuntur, (quorum haud pauci, absque ope, etiam expiraturi fuissent,) hæc sunt in usu." And then, amongst the measures to be employed, he mentions the most generally useful of all, "Subita inspersio aquæ frigidæ in faciem."

"the Court of Exchequer and the jury decided that the child was born alive, because, when it was immersed in a warm bath, a twitching and tremulous motion of the lips appeared twice." But in criticising this conclusion, it is only fair to inquire what was the kind of medical testimony which the Court had to guide it. Was it placed in the dilemma of having to choose between contrary opinions? The matter is so weighty that I shall refer to the trial in question, as I find it cited by Dr. Taylor. It was the object of the plaintiff to show that the child was born alive. "Dr. Lyon, the accoucheur who attended the plaintiff's wife, had died some time before the trial; but it was proved that he had declared the child to have been living an hour before it was born,—that he had directed a warm bath to be prepared, and when the child was born gave it to the nurse to place in the bath. The child neither cried nor moved after its birth, nor did it manifest any sign of active existence; but the two women who placed the child in the bath swore, that while it was immersed, there appeared twice a twitching and tremulous motion of the lips. They informed the accoucheur of this, and he directed them to blow into its throat; but it did not exhibit any further evidence of life. The principal question on the trial was,—“Whether the tremulous motion of the lips was sufficient evidence of the child having been born alive?” The medical witnesses differed. Dr. Babington and Dr. Haighton gave their opinion, that had the vital principle been extinct, there could have been no muscular motion in any part of the body; therefore the child had, in their opinion, been born alive, or manifested life after its entire birth. Dr. Denman gave a contrary opinion: he contended that the child had not been born alive, and attempted to draw a distinction between uterine and extra-uterine life. He attributed the motions of the lips after birth to the remains of uterine life. The jury, however, under the direction of the court, did not adopt this view of the case: they pronounced the child to have been living; and by their verdict the plaintiff recovered the property of which he had been for ten years deprived.”*

* See Medical Jurisprudence, p. 610. It appears from this report that, if Dr. Denman's evidence had been admitted, the husband

The few observations which I shall venture on this history will be made with deference, because of their being directly opposed to deservedly esteemed authority. It plainly appears that the transfer of property, despite long possession, was dependent on the affirmed occurrence of one or two muscular quiverings. The decision rested, be it observed, not on what the medical witnesses *saw themselves*, but on what “two women” reported that they beheld very long back, so that it is impossible to say what was the exact appearance of the movements. There is no reason to suppose that they were voluntary, or in any way connected with consciousness. They were, in all probability, similar to those motions which occasionally hap-

could not have been made a tenant by the curtesy of England. The law draws a clear distinction between the life of the *unborn* and the *born* child. It seems also necessary that the child be born *during the life of the mother*; so that an infant brought quite alive into the world by means of the Cæsarean section, would not enable the father, *unless* the mother were *living* at the time of the operation, to become tenant by curtesy. Blackstone says, “The issue also must be born during the life of the mother; for if the mother dies in labour, and the Cæsarean operation is performed, the husband in this case shall not be tenant by the curtesy—because, at the instant of the mother's death, he was clearly not entitled, as having had no issue born, but the land descended to the child while he was yet in his mother's womb; and the estate, being once so vested, shall not afterwards be taken from him.” (See Commentaries, 12th edit., book ii., p. 127.) By the law of Scotland it seems “that curiality or curtesy only takes place where the issue has been heard to cry.” Even the act of *breathing*, the raising of one eyelid, and subsequent convulsions, have not been held enough. (See Dyer's Reports, vol. i., 25, a.) It must be proved that the child *cried*. But according to the English law, “though crying is the *strongest* evidence of its being born alive, it is not the *only* evidence.” “Some have had a notion that it must be heard to cry; but that is a mistake”—(see Blackstone, *opt. et loc. cit.*)—a point very well shown by the case of *Fish v. Palmer*, referred to above, in which, as has been stated, the circumstance of two muscular twitches happening upon immersion in a warm bath, and *ten* years previously to the time of trial, and sworn to, not by any medical witness, but by two females, “the nurse and the cook” (as one report declares), were held sufficient evidence of “living,” and to justify a decision in the plaintiff's favour. It may be well to add a passage from Blackstone on Tenancy by curtesy, in order to save the curious reader the trouble of making reference. “Tenant by the *curtesy of England*, is where a man marries a woman seised of an estate of inheritance, that is, of lands and tenements in fee-simple, or fee-tail; and has by her issue, born alive, which was capable of inheriting her estate. In this case, he shall, on the death of his wife, hold the lands for his life, as tenant by the curtesy of England.” See Stephen's Commentaries, Vol. i., second edition, p. 251. See also Beck's Medical Jurisprudence, seventh edition, p. 114. I had not seen the observations in this work, at the time of making the remarks which I have ventured.

pen in the completely dead, speaking of death as we generally view it. They were rather, as I think, evidences of *past* than of *present* life, looking at that complex, perfect life, the life of the body taken as a whole, and as distinguished from mere *local* vitality. Shall we consider *such* quiverings as signs of life, and not go one step beyond, and consider as manifestations of life also those movements of the muscular fibre which may be produced by galvanism long after the latest pulsation of the heart? So long as there is the faintest respiratory movement, or the least perceptible pulsation of an artery; so long as a beat at the heart can be felt, or the weakest sound of it be detected by the stethoscope, a person must be called alive, though just on the verge of death: but is he to be called living because a muscular fibre may chance to quiver? Are we to say of those cholera subjects who manifested such protracted contractions that they did not die till the last was over?

To me the distinction which Dr. Denman drew between uterine and extra-uterine life seems a good one, and that which should have guided the Court.* Had the child performed but one act of inspiration the case would have been different; it would have given manifestation of a *vital* function, as we commonly express it, and have made, moreover, a movement *peculiar to extra-uterine life*. Two twitchings of the lips, which *might, perhaps, have never happened but for the impression of the heat*,† were held sufficient to decide the Court. The law of Scotland would, I am informed, have required proof of crying as evidence of respiration. It may be well questioned whether the question of life or death ought to turn upon the existence or absence of a muscular tremor. We must not, in fairness, call a quivering of this kind a sign of life sometimes, and then reject it at other times as an indication of "living," when it may square with our theories, or suit our convenience so to do. Whatever may be the view adopted by the reader, he will, I am sure, feel the necessity of considering the whole subject of what have been termed post-

mortem contractions before positively concluding. He has not simply to ask himself what is life in a scientific and abstract point of view, but to consider what it is as the word is generally employed.*

The action of the involuntary muscles after the annihilation of consciousness, and the cessation of the breathing, has now been very long observed. It is necessary to distinguish between the action of the heart, or intestines, or uterus, and that mere quivering of fibres, which in nowise reminds one of the special and perfect action of these different structures.

The state of the heart, like that of the voluntary muscles, is modified not by death simply, but by its kind: abrupt dissolution and slow decay leave it, no doubt, most oppositely irritable.† The physiologist must be cautious of applying unguardedly the observations made by him on one form of death to another form of it which is completely different.

It is to be reasonably expected, looking to the results of experiments on animals, that the heart in the human subject must quiver, more or less, after some violent and sudden deaths, when neither the touch can discover, nor the ear detect, how attentive soever, the least proof of action; and that the intestines must still persist in their vermicular contractions. Dr. Feldmann observed the heart of a decapitated criminal; the right auricle showed marked contractions on the pericardium being removed. Vesalius, as every one knows, is reported to have remarked a similar phenomenon in the dead body of a Spanish nobleman, and to have been, despite his rare and ardent devotion to anatomy, absurdly and ignominiously banished from a country which should have held him punctiliously in selectest honour.

Intussusception, of which there was no evidence during life, has been again and again discovered after death. Rokitsky is of opinion that it "occurs during the last moments;" but I think it not unlikely that it may also happen at a later period than that phrase implies. It results, according to him, from unequal irritability of the intestine—an

* Yet how could the Court have decided otherwise, in the face of two such able and distinguished witnesses as Dr. Haighton and the late Dr. Babington?

† I have seen muscles contract on the direct application of heat to their fibres.

* The reader will find some definitions of life in a note in the 3d volume of Mr. Palmer's edition of Hunter's Works, p. 126.

† This has been forcibly insisted on by Mr. Travers in his "Inquiry."

ingenious idea! for it is easy to suppose that one part of it may be lax and passive, whilst another, directly above, is in a state of active contraction. Probably the different states of irritability in different parts of the intestinal canal, which we cannot suppose to be without exception uniformly irritable throughout, may occasionally give rise to intussusception in life-time also, and explain, moreover, certain inequalities or irregularities of rigor mortis. As to the latter point, it may be inferred that the most irritable portions, being the latest to contract, and remaining contracted the longest, may now and then be seen by the pathologist in the form of those partial constrictions which seem more satisfactorily explained by a reference to the effects of rigor mortis than they can be otherwise.*

The time at which rigor mortis of the heart comes and goes no doubt varies extremely; and the organ is most likely to be found rigid in those cases in which it approaches tardily and lingers

* The reader may compare the observations of Rokitsky on the production of intussusception with those which have been stated by John Hunter.

Rokitsky says:—"We naturally ask how the intussusception is brought about, and how its enlargement is effected?"

"The cause is to be found either in the contraction or moveability of a piece of the intestine, on which account it passes into the adjoining or more capacious tube; or in the extreme expansion or relaxation of a segment of intestine, which gives rise to an inversion of the adjoining narrower or more innervated portion."—Rokitsky's *Pathological Anatomy*, printed for the Sydenham Society, p. 56.

Hunter observes:—"The manner in which it may take place is, by one portion of a loose intestine being contracted, and the part immediately below relaxed and dilated, under which circumstances it might very readily happen, by the contracted portion slipping a little way into that which is dilated; not from any action in either portion of intestine, but from some additional weight in the gut above. How far the peristaltic motion, by pushing the contents on to the contracted parts, may force them into the relaxed, I will not determine, but should rather suppose it would not."—See Hunter's *Works*, vol. iii., p. 587.

Rokitsky would seem to imply, by the phrase "more innervated portion," that the narrowing of the portions of gut which become involved is owing to its being more contractile than that part of the bowel which it enters. How far intussusception may be explained by the unequal irritability of different portions of the intestinal tube will be disputed no doubt; but, unlike what may be said of some assigned causes, it seems capable of explaining the effect in question. It may be imagined that opium could not be given without risk for intestinal spasm occupying some fixed and limited spot, if it had the effect of exclusively paralyzing the affected part without in any way influencing the contractile power of that immediately above it: a condition would in this way be produced very favourable, it may be presumed, to intussusception.

long. In some cases it may be presumed to have come and gone before the time of the examination of the body.

The *peristaltic* action of the uterus may very likely endure, in some instances, after respiration has ceased, and so of itself complete a delivery.* I know not whether any observations have been made on rigor mortis in relation to the uterus; but certainly it will not be proper to infer that this organ was contracted before death simply because it is beheld contracted after it. This point must be borne in mind in the examination of women who untimely perish in the parturient state.

Interesting as is the condition of the involuntary muscles after death, and instructive as it would be to consider it more fully, I must leave the subject now. It could not have been properly omitted altogether, because the state of the involuntary muscular fibre is as much modified as that of the voluntary by the various modes of dying. A very good illustration of the opposite conditions of the muscles, left by unlike kinds of dissolution, is given by Sir Gilbert Blane, in the Croonian Lecture on Muscular Motion which he delivered before the Royal Society in 1788:—"It is a curious and well ascertained fact, that if a fish, immediately upon being taken out of the water, be stunned by a violent blow on the head, or by having the head crushed, the irritability and sweetness of the muscles will be preserved longer than if it had been allowed to die with the organs of sense entire. This is so well known to fishermen, that they put it in practice in order to make them longer susceptible of the operation called *crimping*. A salmon is one of the fish least tenacious of life, insomuch that it will lose all signs of life in less than half an hour after it is taken out of the water if suffered to die without any further injury; but if, immediately on being caught, it receives a violent blow on the head, the muscles will show visible irritability for more than twelve hours afterwards."

It may be stated that, in all cases of sudden death, unless they are such as surprise persons either debilitated by

* I have lately, in an experiment, seen the most lively action in the uterus of an animal after it was removed from the body. It is quite clear that the organ can act independently of the spinal cord; but it is thrown into action, as many observations prove, through the reflex function of that organ.

long disease, or extremely weak from some cause or other, that the muscular fibre is extremely irritable, although it may not manifest irritability without some stimulant being applied. Abundant evidence of this fact has long been given by physiological experiment. The only wonder is, that the spontaneous mobility of the fibre after certain cases of quick destruction has been so tardy in attracting notice.

I have in a former paper alluded to the twitches of the muscular fibres seen in slaughtered animals. They are matters of popular, loose observation. I have observed them in mice which I have destroyed by carbonic acid and hæmorrhage. I have seen them in frogs many times. They do not appear to be any the more powerful, judging from what I have observed in the latter, from strychnine being administered previous to decapitation. This tetanus-producing poison never, as it seems, acts upon the muscular fibre, unless through the medium of the spinal cord.

As the state of nutrition of the fibre at the period of experiment, and the way wherein it may have been circumstanced as to rest and exercise, make every difference in regard to the occurrence, extent, force, and duration of the quiverings of the fibres, the latter may, in some sense, indicate in what condition the muscles were, in relation to those matters, at the time of dying. I will sketch an experiment, with the view of illustrating the prevention of any manifestation of irritability by the impoverished state of the muscles. I took a frog which seemed extremely languid, and removed the head quickly; on stripping off the skin, no muscular quiverings were seen anywhere—no, not the faintest: the muscles were extremely pale, flabby, evidently ill-nourished, and some spots were seen here and there upon the fibres, looking not unlike dots of fading purpura.

In observing the state of animals killed by division of large blood-vessels, it is obviously necessary to well distinguish between those *immediate* convulsive motions which depend apparently, as Dr. Marshall Hall has pointed out, on centric irritation of the spinal cord, and those remoter quiverings, very slight and very restricted often, which seem referable to the state of muscular fibre. And here let it be said that there cannot by possibility be a greater con-

trast of appearance than that between those quick and universal motions which I have seen in animals destroyed by hæmorrhage, and those peculiarly wayward, partial, shifting, often extremely slow movements—quite alike, perhaps, in no two cases—that I have beheld after cholera and apoplexy. No one *could* confound them, unless by actual purpose: they are unlike in form, in rate, in period of occurrence often,—unlike, in short, in almost every thing except in being manifested through the muscular fibre.

It may, in the present state of our knowledge, be disputed whether motions of the limbs—such, I mean, as *actually change their position*—do really occur in sequence of death by hæmorrhage, after those convulsive motions are fairly over which directly follow the fatal loss of blood. It may, perhaps, be anticipated, supposing keen inquiry be made, that distinct motions of this kind will, though perchance rarely, be discovered in such a case; but, in watching for such, the greatest care must be taken not to mistake any reflex actions externally excited for, and confound them with, these. I would here refer to some remarks of M. Bouchut, which he makes in speaking of the immobility of the body as an immediate sign of death.*

“L'immobilité du corps, c'est-à-dire la cessation des mouvements dans les membres, est un résultat incontestable de l'abolition des fonctions du système cérébro-spinal. Toutefois ce phénomène ne saurait être considéré comme un indice certain de la mort de l'homme, puisque dans un assez grand nombre de maladies nerveuses, l'épilepsie en particulier, il peut y avoir un instant cessation des mouvements musculaires, qui reparaissent lors du retour à la santé. De plus, si les muscles sont immobiles dans leur totalité, dans leur ensemble, de manière à empêcher tout mouvement des membres, leurs fibres se contractent encore partiellement plusieurs heures après la mort, surtout dans les cas de mort violente. C'est ce que Nysten a parfaitement bien établi par de nombreuses observations faites chez les suppliciés. On pourra les vérifier en assistant au travail des bou-

* See *Traité des Signes de la Mort*, p. 145. It is quite plain that there may be death without immobility, and immobility without death, as cholera and syncope, to go no farther, prove absolutely.

chers, qui, dans leurs abattoirs, sont tout surpris de voir les fibres musculaires se contracter encore chez un bœuf entièrement dépecé, coupé en deux, et *monté*, comme ils disent, environ près d'une heure après la mort. Ces contractions, qui seraient incapables d'imprimer aucun mouvement aux membres, ont cependant lieu dans les muscles de la vie de relation, c'est-à-dire dans les muscles volontaires, et sont utiles à indiquer, lorsqu'on parle de l'immobilité du corps après la cessation de la vie."

It is quite clear that the observations of Nysten on the great irritability which so long remains to the muscular fibres after death, and the abiding and pertinacious quiverings in slaughtered animals, have not yet been sufficiently applied to pathology. Looking to the principle on which they may depend, it would be most likely erroneous to separate the slight quiverings which merely oscillate on the surface of the muscles, without stirring the limbs, from those strong and striking contractions of them by which the legs and arms are again and again variously moved in different directions, and, *occasionally*, in a manner which resembles, *at first sight*, that of the movements which spring from volition. One law, it is probable, includes the two classes of instances: and we may well believe—for there is much evidence towards this conclusion—that in cases of sudden and speedy dissolution, subcutaneous muscular quiverings are infinitely more common than is generally supposed, though, from their feebleness, or the thick covering of integument which invests them, they are invisible. We do not see the muscular twitchings of destroyed animals before we strip their skins from off them; and it is plain enough occasionally, that when we do see them, that we should have no chance of discerning them unless we first laid the muscles bare. Lately, in watching the twitching of the muscles of an amputated leg, I failed to detect any save in those fibres which were exposed: these were most lively, were renewed at intervals, just as the after-death contractions of cholera are, and were readily excited by pricking or percussion, just as these may sometimes be. It may, indeed, be contended that the exposure to the air excites these quiverings, and that there is no proof whatever of their

existence prior to their being laid open to its influence; but it must be remembered that twitches have been seen after mortal asphyxia (in which, be it observed, there must have been *less* of oxygen in action upon the fibre than there generally is) without the skin being interfered with, and the most distinct contractions have, as has been amply shown, repeatedly affected uncovered muscles. More, too, is often needed to produce motion than exposure to the air, even where the muscles are very irritable; for frequently all the exposed fibres do not contract: one or two muscles only may be affected, and very restricted portions of these even. Why are these quiverings sometimes so limited, so partial? Are the particular fibres which are active more irritable than the rest, or are they influenced by some stimulus confined to them alone? In speaking thus of the asserted influence of the air, I am very far from saying that it has no power of any kind to cause or to maintain contractions. I would only go the length of asserting that it does not seem at all necessary to bring about the activity of the fibre in all cases, though in some it may rouse the still muscle, and make the active keep disquiet longer than it would otherwise have done.

Although we cannot, in considering the whole subject, draw an impassable line between the slight quiverings and the remarkable motions, it still remains difficult to explain the reason of the extreme degree of contraction which prevails in some cases. Some aid, as has already been insisted, is undoubtedly afforded by making proper allowance for the various states of the muscular fibre, as to its amount of irritability at the time of death.

Those states, however, will not account for everything. The question has been asked, in previous observations, whether the muscular fibres, or terminal motor nerves supplying them, may not for some time after death be still irritated, in cases of cholera, by the poison of that disease, or the changes of blood it causes, whatever it or they may be? It seems the more reasonable to ask it, looking to what has been remarked in some poisoned animals. Yet we must be careful of attributing any *peculiar* power to the supposed poison of malignant cholera, seeing that remarkable motions have also followed *yellow fever*,

and keeping in mind the state of the muscles in the case of *apoplexy* which I have described. This case is, indeed, of especial interest, when we come to argue on the morbid stimuli which, after the cessation of respiration and circulation, may be still supposed to influence the excitable fibre.

As to the slight undulatory motions which play about the muscles of animals just killed by loss of blood, it is quite clear that these cannot *possibly* be imputed to any morbid material. Does the sudden *withdrawal* of the impression of the circulation on the muscular fibres, or their minute motor nerves, ever lead to quiverings, just as the sudden loss of blood circulating through the spinal marrow has been held to cause general convulsion?

In our consideration of the property of muscular irritability, we must never forget how variously it may be excited, and how inexpressibly difficult it really is, not so much to affirm what its excitants are, as to assign to them their due and relative power. Its manifestations after death, as during life, may depend clearly on different causes: now powerful though unseen stimuli may occasion them, though the measure of irritability be not extreme; now slight stimuli may provoke them, because of its amount being excessive; and again they may exist in unusual force, both on account of the power of the exciting cause and the rarely responsive condition of the fibre whereupon it has to play. And though I incline to the belief that the striking contractions which now and then happen after fatal cholera do really no more depend upon any excitement of the spinal marrow, centric or eccentric, than do the quiverings of an amputated limb—which of course cannot be, by any one, referred to it—I confess it far from *certain* that movements after death in the human subject are *never* referable to impressions made, directly or indirectly, on the spinal cord.

It would save a vast deal of trouble, and appear, at first sight, very consistent, to affirm at once and broadly some simple general principle, styling it a law, and then support it by very unceremoniously twisting some facts, and alone resting on those phenomena, with a cautious partiality, which point in unison towards a particular direction; but this mode of proceeding is not

even to be thought of, and the inquirer must rather confess his difficulties, and wait patiently for further evidence, ere he decide on the power and relative power of supposed morbid excitants of the muscular fibre after extinguished life. Meanwhile, however, let him try to explain as much as he is able; more facts will be found in time; and what are now uncertain glimmerings may in the end expand, and brighten into a full, clear light.

The main circumstance in relation to these post-mortem movements is, so far as we can see at present, undoubtedly the *suddenness* or *rapidity* of death: yet, no sooner do we try to account for their occurrence (I speak now of *actual* movements of the limbs), by quick dissolution, than we are met at once by the observation, that as yet they have been seen only after a *few* instances of swift loss of life. This is certainly true when we come to consider how very seldom, except, indeed, in cases of cholera, they have been observed. As yet, so far as I know, they have been noted in *one* case of apoplexy only, although this is a disease in which one might think they would have been more commonly beheld. At present there is a dearth of instances.*

The subject is rendered much easier by viewing the slight quiverings, and the changes of position, merely as phenomena which differ in *degree*: still that difference of degree is so extremely marked, that it is not a little difficult to clear up, even on making all those allowances for variations in the condition of the muscles which may be presumed to exist at the last moment. It is one thing—let the remark be repeated—to say that a muscle is so irritable that the least stimulant will act,

* I find the following passage, by Dr. Hamett, in the substance of the Official Medical Reports on Cholera as it prevailed among the Poor at Dantzick between the end of May and the first part of September, 1831. Alluding to the "morbid effects," he says:—"The next is the inviolable contraction of the bladder; and another, which, although not apparently constant after death from this disease, is seldom or never to be met with after death from others—namely, slight spasmodic contractions or movements, if they may be so called, in the muscular fibres here and there in the body, and more especially in the face and extremities, not only immediately, but some time after dissolution." From the use here made of the word *slight*, and the expression, "if they may be so called," applied to *movements*, it would appear that Dr. Hamett did *not* observe the most remarkable kind of contractions which are known to happen. (See the above substance of Report, p. 51).

and another to state clearly the cause of its action. Yet it is certainly a great step to see plainly some conditions which favour, and some circumstances which preclude, any visible manifestations of post-mortem irritability; and no little thing to be able to disabuse our minds effectually of the notion that such as the more remarkable are peculiar to cholera. It *may* be that they have been so much more frequently observed in this disease from the sad circumstance of its ruthless mortality, which, unhappily, affords us such a multitude of opportunities of examining bodies robbed speedily of life.

It may be said, that if muscular tremors are not seen after death in cases of chronic maladies, wherein the vitality of all the tissues is impaired exceedingly before dissolution, that they are at times beheld very frequent and palpable on the verge of its event, as—to take a good and familiar illustration—in typhus fever. True: but in typhus fever, and some other cases wherein such tremors occur, I apprehend that their presence may be satisfactorily accounted for by supposing the muscles to be agitated by mental influences on the one hand, whilst imperfectly controlled by volition on the other.*

The quivering of the fibres of an amputated limb is as good an instance as can well be given of the action of muscles independently of any influence of the nervous centres; for it takes place under circumstances of separation from them. It may be regarded as an elementary fact ever to be borne in mind in the consideration of certain obscure cases of muscular contraction which we are utterly at a loss to explain. There are instances occurring in which no one, let his acumen and his knowledge of nervous and muscular phenomena be what they may, who could say for *certain* of *some* forms of spasmodic action, whether they be centric, eccentric, or referable to some irritation of the muscle itself.

There is, if I mistake not, as much difficulty in accounting satisfactorily for the state of the muscles in cholera during life-time, as in explaining conclusively the various phenomena presented by them after death.

1. Are the cramps produced reflexly

* See an Essay by the writer on Some of the Relations of Volition to the Physiology and Pathology of the Spinal Cord, MEDICAL GAZETTE, vol. for 1848.

by irritation of the mucous membrane of the intestine?

2. Or are they occasioned, centrically, by the blood circulating through the spinal cord?

3. Or are they brought about by changes of the blood-current in the muscles themselves, affecting their fibres or motor nerves?

These are questions which, in my humble opinion, are far more easily asked than answered. They may be as yet pronounced open questions.

I much hesitate to subscribe to the view which has been entertained of their being reflex, because of some points attaching to their form and course, and because, also, of the remarkable rarity of general convulsion in that stage of cholera wherein they exclusively occur.*

On the same grounds, too, I think it very doubtful whether they be of centric origin. It is, however, known well enough that irritation of the cord will produce cramps; and the way in which they not unfrequently forerun, and, viewed properly, foretell incipient diseases of the spinal marrow, is an interesting illustration of the fact. If the altered circulation of the cord itself did really provoke them, is it not inexplicable why the glottis should not be sometimes closed, and the patient either perish from, or be imminently endangered by, rapid asphyxia?

The third question cannot, viewed closely, be replied to with a ready and unconditional affirmative, though I think there are facts which make it very probable that the seizures of the muscle in cholera depend upon either *direct* irritation of its fibres or the terminal motor nerves supplying them.†

One sees a particular muscle in rigid and long maintained action, when—not seldom—scarcely any other is affected at the time. If this contraction be from centric irritation of the cord, it must obviously be applied most partially. When altered states of the blood produce spasms, they generally produce such as are general,—such, in brief, as the poison of scarlatina, or of measles, or variola, brings severally about.

* I have never seen what could be properly called a *general* convulsion in the cold period of the disease. Once in the *subsequent* feverish stage, I saw a patient who had some time been comatose die in a true fit of epilepsy.

† Dr. Wilson has very especially insisted on the action of the blood on the muscle itself as a cause of spasm. See his work on "Spasm, Languor, and Palsy."

No doubt centric irritation will occasionally give rise to most limited movements; but when it does so, it is by influencing a *particular spot* originating fibres which are distributed to the muscles, or muscle, or even portion of muscle, which may be affected. Reflected irritation may be held more reasonably to explain these cramps; but this mostly displays itself in a different form to that which marks the cramps of cholera, and is so prone to produce general convulsion, that one, as I have said above, can scarcely wonder that it does not do so in cholera at times, if really it be the cause of the muscular disturbances. But why, as the blood, poisoned throughout, pervades *all* the muscular fibres of the body, are not the cramps *universal*? Because, it may be, those fibres are variously irritable. Galvanism will not affect, in some cases, all the muscles of the body equally. Cramp has an extraordinary partiality for the muscles of the calf of the leg. Certainly there are none, as a general rule, more often used, more fully nourished, than these are; and there are none which may be supposed in a condition, therefore, to respond more lively to any morbid impression.

Why, again, if the muscles be so influenced by the blood as to contract, and even become pertinaciously rigid, is the *heart*, the principal and most irritable of all, exempted from a seizure which would soon destroy? It is in vain for me to essay an altogether conclusive answer to this hard inquiry; but let it be here well remembered that a stimulus—galvanism to wit—which may most powerfully affect the voluntary muscles will very inferiorly influence the involuntary.*

We have been hitherto far from fully appreciating, and still farther from fully applying, the facts which we know relative to the excitement of muscular action in demonstrable independency of the nervous centres: I say not of the terminal motor nerves.

The effect of altered states of the blood in producing general convulsion is clear enough, though by no means at present adequately investigated; but far less explored has been its influence in the

production of cramps and certain partial spasmodic maladies. "A fit of the gout," says Heberden, "has been judged to suspend the power of cramps; but I am much more strongly convinced that the gout is apt to breed and foster them."*

One of the most remarkable circumstances in the history of cholera is the different degrees of cramp. Some, during last year's epidemic, fell unresistingly, and experienced hardly a pang: they died as people overwhelmed by violent, irrecoverable shock. Others were racked most piteously: no trait was wanting to make more perfect their finished picture of severest agony. The muscles, defying all rule completely, were gathered here and there into hard, conspicuous knots; the limbs were strangely distorted, and effectually fixed; the features, just, perhaps, as the will would have directed, or emotion influenced them, were locked rudely in terrible grimace; and the enfeebled irritability of the seats of spasm gave the only respite.

The whole state of the nervous system also varied greatly. Let us for a moment bring to memory the condition of the brain. Some lay in a silent prostration, which might, perchance, be mistaken for coma at first sight,—lay, not moving a muscle, save when some pain distressed, some want was urgent. Their case, not without parallel, was comparable to that which we see almost daily in fatal burns, wherein the mind and body seem prostrated together and alike.† But in other instances there was, on the contrary, an extreme restlessness, which gave way to nothing, save one—mortal anodyne,—a fruitless, unrestrainable exertion to be remarked often in the sinking state, and making it the fleeter. I was much struck during the past epidemic by many instances in point; amongst others, by that of a young Irishwoman, who, in the prime of life, and, to all appearance, most desirous of living, rapidly perished. Hers was one of the too numerous cases which reminded us, "*remedia tardiora quàm mala, et ut corpora lentè augescunt citò extinguntur.*"‡ Her lively temperament, contrasting with the pal-

* L'irritabilité est la même partout; elle ne varie qu'en intensité dans les différens muscles, mais elle n'obéit pas aux mêmes *stimulus* dans tous les muscles.—See Œuvres de Legallois, tome i. p. 222.

* See Commentaries, fourth edition, p. 342.

† See An Inquiry, &c., by Benjamin Travers, F.R.S., p. 107, and Leçons Orales de Clinique Chirurgicale, par M. le Baron Dupuytren, tome i. p. 436.

‡ Tacitus, Vita Agricolaë.

pable, fatal collapse, was manifested from time to time by a singular and marked vivacity of manner, by "an inquietude and restlessness which aggravated the ineffable prostration of her strength."* It was, indeed, impossible to help closely contemplating the mental condition of some patients, with their circulation so shocked; their respiration so imperfect; their blood so damaged. Wonderful to see the signal difference between the mind and body! Wonderful to see thought, will, emotion, thus active, thus enduring! Could we imagine some livid corpse to speak, move, "glare with unspeculative eye," we should hardly picture a more marvellous spectacle than the mind unsubdued by the destroying pestilence,—when the keen anatomy of the features dismally shrunken as by long disease; the body shrivelled, livid, earthy cold; the hoarse, almost inaudible voice, now whispering of the quenchless thirst, now of the torturing cramp; the sighing, irregular, long-pausing breathing, ever threatening to fail; the languid heart; the pulseless wrist; and all the symptoms of the disease combined to prognosticate swift dissolution.

Let me now for the present conclude these observations, which have reached already to an unexpected length. Whatever errors of reasoning may be found in the few comments I have ventured, will, I hope, in consideration of the manifest difficulties of the subject, be viewed indulgently. Every phenomenon which manifests or illustrates that force whereby the muscles immediately contract is of considerable interest: and the (so-

called) spontaneous manifestation of it after death is of unusual attraction, owing to the circumstances under which it occurs. Unexpected by the generality of observers, it is apt to take them at a disadvantage, and, perchance, to hurry some of them into erroneous inferences.

But the whole subject of that power whereby the muscles act directly, deserves, though it may obtain not, the most general study. No wonder that it so occupied the severe attention of the great intellect of Haller! Imagine the innumerable fibres, voluntary and involuntary, which, through changes so infinite, and a time so protracted, it never refuses to supply with energy! Imagine the sources of its renewal, the modes of its disturbance, the causes of its exhaustion, and all the purposes whereunto it is devoted and adapted—some vital, others essential to life's noblest ends! Mark it exemplified in the contractions of the heart, the expression of the features, the working of the hand; now note it in the tardiest thing that crawls, now in the swiftest that flies through viewless air! Think only of the manifold uses to which it is converted throughout the life of animals in its every form! Can we ever behold with incurious eye any demonstration of this pervading, indispensable force, wherever or whenever we may perceive it? Shall we not rather diligently contemplate it under every circumstance, and try to class truly the minutest movement which it possibly can cause, or which can be caused by means of it, no matter how unmeaning it may seem at first sight, with those of its own kind?

* Tallman as cited by Dr. Chambers.

Westminster Hospital, Oct. 9, 1850.

