

PART III.

MEDICAL MISCELLANY.

On Cadaveric Rigidity, and the Conditions of Muscular Rigidity nearly allied thereto; with special Reference to State Medicine. By Dr. ADOLF KUSSMAUL, Lecturer on Medicine at Heidelberg. Abridged from the original by W. D. MOORE, M. B.

THE author shows that an individual is to be considered as dead so soon as the medical jurist has satisfactorily convinced himself of the permanent cessation of the heart's action. Among the changes which the body undergoes after death, the so-called cadaveric rigidity is one of the most striking and important. The seat of this phenomenon is, as the simplest experiment will easily prove, in the muscular tissue. The stiffness of the limbs, the immobility of the joints, is, as Nysten first demonstrated, not destroyed when these parts are deprived of their skin; it continues, moreover, even after the lateral ligaments of the elbow or knee-joint have been removed, the synovial capsules opened, and water injected into them. On dividing the corresponding muscles, on the contrary, it instantly disappears. The muscular tissue of the body usually passes through three several stages to its complete disintegration. At first, flaccid and contractile, it subsequently becomes rigid and incapable of contraction, until it finally once more relaxes, of course without regaining its contractility. Of these, it is only the rigid condition which enters into the following considerations.

The rigid muscle possesses a great but imperfect, while before becoming rigid it possessed a slight but perfect, elasticity; it has consequently become less extensible. Its cohesion is diminished. Microscopically the transverse striæ appear more distinctly. At first it contracts weakly, but soon not at all, on mechanical, chemical, or electrical irritation. The parenchymatous fluid no longer contains any free oxygen, the muscle has consumed it and breathes no more; its living metamorphosis has ceased; in a word, *the muscle becoming rigid is a dying, the perfectly rigid is a dead muscle.*

Nysten considered cadaveric rigidity to be "*the last effort of life against the action of the chemical forces, which tend to dissociate the elements of the organization.*" To this hypothesis the Danish phy-

siologist, Sommer, in 1833, objected that it bore the stamp of improbability that a force near its extinction should experience so immense and persistent an increase. At the present day, it may suffice briefly to point out three of the most important and most accurately established distinctions between the rigid and the vitally contracted muscle. The contracted is more, the rigid less, extensible than the muscle at rest. Strong electric currents traverse the contracted muscle, but in the opposite direction to those in the muscle at rest; in the rigid muscle, on the contrary, they are much weakened, and soon entirely disappear. Finally, the metamorphosis of tissue is increased during contraction, but annihilated in rigidity.

To Nysten's vital hypothesis, Sommer opposed his physical theory. Cadaveric rigidity, according to him, is not a living—it is a *physical contraction*. Muscle possesses a vital and a physical contractility, but the latter is not distinctly manifested until the former is extinguished. Like Nysten, Sommer clings to the view that an hypothetical peculiar vital power in the system, to a certain degree, keeps in check, limits, and removes the physical and chemical forces inherent in matter; but, to refute this part of his theory, it is sufficient to show that the essence of cadaveric rigidity does not in general depend upon a contraction of the muscular fibres. Among other more striking proofs of the truth of this may be mentioned the fact, that the limbs become rigid in whatever position is given to them. In flexed limbs, not only the shortened flexors, but also the lengthened extensors, stiffen. The error of considering rigidity as physical contraction arises in great part from confounding contraction with increase of elasticity. The limbs are not immovable, because, as in convulsions, a shortening of the muscular fibres has taken place, but because the latter, by reason of their greater elasticity, oppose an increased resistance to the movements.

The theory mooted in 1842 by E. Brücke, in returning to the opinion of those who attributed the rigidity to *coagulation of the juices*, was received with much favour. But as the simplest anatomical investigation is sufficient to prove that it proceeds neither from congelation of the fat and synovia, nor from the solidification of the fibrine in the capillary vessels, but that it is situated in the muscular tissue, he endeavoured to show that the latter is during life constantly infiltrated with fluid, fibrinous, nutritive material, which afterwards coagulates in death, precisely as the fibrine of the blood does in the vessels. But it was found that the injection into the vessels of fluid capable of preventing the coagulation of the fibrine of the blood did not prevent the occurrence of cadaveric rigidity; hence this theory also appears to be untenable.

The author details some experiments, from the results of which he infers, in opposition to the views of Engel and Stannius, that the death of the nerve is not a necessary preliminary to the occurrence of rigidity in the muscle. They had advanced the opinion that *cadaveric rigidity represents that elasticity of the muscle which it possesses when completely removed from all nervous influence*.

The theory which at present appears to offer the most satisfactory explanation of the occurrence of cadaveric rigidity is that which originated in the investigations of Edward Weber, namely, that the alteration in the elastic condition of the muscle, on which that phenomenon depends, proceeds from chemical changes in the muscular fibrine.

The author next inquires in what point of view cadaveric rigidity may become an object of interest to the medical jurist, and in the examination of the subject adopts the following arrangement:—

1. What value is to be assigned to cadaveric rigidity as a sign of death having taken place.
2. What data does it afford to calculate the time which has elapsed since the death of an individual?
3. What inferences can be drawn from its degree and duration, as to the constitution, disease, and mode of death of the deceased?
4. To what errors may it give rise in the examination of the remains?

In reference to the first question the author observes, that there is not to his knowledge a single well-established case in which cadaveric rigidity was entirely absent. It has indeed been said, that in consequence of certain morbid conditions having existed, it often does not set in; among these are enumerated, by various writers, putrid diseases, rachitis, phthisis, and asphyxia from the inhalation of carbonaceous vapours; but such statements have probably originated in imperfect observations. The author's experiments completely refute the assertion that rigidity does not occur in cases of narcotic poisoning. Dr. Kussmaul has been unable to find any reliable authority for the statement, that it is absent in animals hunted to death, and a similar assertion, as to individuals struck by lightning, is refuted by experiment and observation. Equally unsatisfactory are the accounts which have been published of its *imperfect* occurrence in prematurely expelled fetuses, while Bertelsen and Ehrmann have witnessed cadaveric rigidity in fetuses which were extracted, when nearly at the full term, by means of the Cæsarean section. It is to be regretted, adds the author, that we are still quite uninformed as to the period at which the muscular fibre of the embryo is so far developed as to be capable of becoming rigid.

With greater certainty is the fact established that *cadaveric rigidity sometimes does not attack particular parts of the body*, as, for example, the shattered limbs in cases of death after comminuted fractures, or the paralyzed, badly nourished, and water-infiltrated side of an hemiplegic. Stannius found that the limbs which were rendered rigid by tying the arteries in the living animal, but which on restoration of the circulation had once more become movable and regained their irritability, after death did not again stiffen, but passed directly into a state of putrefaction, while the other limbs were perfectly rigid. The author made the same observation in respect to rigidity of the hind-legs produced by the injection of chlo-

reform, when they had again become relaxed during the life of the animal. It hence follows, *that as rigidity may be wanting in particular limbs of the dead body, it may on the other hand be present in particular limbs of the living*. And in this respect the effect of too high or too low a temperature as a means of producing local rigidity resembles that of cutting off the influx of the blood, or of injection with the most widely different chemical agents. Thus, in cases of exposure to the action of frost, the external parts will, *ceteris paribus*, become rigid with the greater ease, and the internal with the greater difficulty, the more the blood has been driven from the periphery towards the centre. It may, therefore, happen that the limbs of a person shall be already stiff and apparently dead, or even in part quite dead, while the heart still beats and the death of the individual has not yet ensued. "I therefore," adds the author, "see no reason why the cases of the restoration of frozen and drunken persons found stiff, recorded in the Transactions of the Royal Humane Society, should be looked upon as so incredible as Sommer thinks them to be."

It is evident from these considerations, as well as from our investigations on the nature of cadaveric rigidity in general, that its value as a sign of death having ensued suffers some prejudice, because, leaving out of the question that the rigidity itself represents only a condition of the apparent death of the muscle:—1. *The constant occurrence of muscular rigidity in dead bodies has not as yet been established for all cases*. 2. *The duration and intensity of the same are frequently so diminished that it easily escapes observation*. 3. *While particular limbs may stiffen in the living body, in the dead they may on the contrary pass directly into putrefaction without becoming rigid*. 4. *Because the possibility of at least partial stiffening of all the limbs and even of the interosseous muscles of the trunk, with persistence of the heart's action and capability of reanimation, is not at present to be established with certainty*.

It now remains to examine whether certain pathological conditions may not simulate cadaveric rigidity, and cause apparent to be mistaken for real death. These will of course be only such as are characterized by a similar immobility of the joints, stiffness of the limbs, and increased tension (apparent hardness) of the muscles. As such we must consider contractions, particularly as they occur in encephalitis and myelitis, tetanus, and catalepsy. The question is, whether we possess simple and certain practical *points d'appui*, which may enable us in doubtful cases to distinguish cadaveric rigidity from these conditions.

Cadaveric rigidity, as it occurs under ordinary circumstances, is characterized by the great regularity of the course it follows in attacking the several parts of the body. It begins almost always in the neck and lower jaws. Sommer saw, among two hundred dead bodies, only one exception to this rule. From the neck it passes, not, as the systematic works agree in stating, in one direction from above downwards, but in two directions, *that is to say, it at the same time*

advances upwards to the muscles of the face. In its course downwards it usually attacks the muscles of the trunk and of the upper extremities; then, those of the lower. In the particular limbs, too, it proceeds from above downwards. It generally passes off also in the same order.

A second characteristic peculiarity consists in this, that the rigidity almost always sets in, increases, and decreases, imperceptibly and gradually. But the following behaviour is still more important. *When a limb affected with cadaveric rigidity is forcibly bent, the rigidity is removed. If it had been fully developed, it attacks this part no more, but if it was in course of development, it returns, as Sommer showed, but only gradually.*

Contraction is characterized by the excessive tendency to distortion, such as under ordinary circumstances never attends rigidity. *If a limb affected with contraction or tetanus is forcibly brought out of its position, it immediately returns to it with violence.* Contraction, too, is seldom persistent, and tetanic stiffness never continues so uniformly; they are more apt to alternate with slighter convulsions; the tetanic rigidity often remits, and returns suddenly with violence, particularly when the sensitive nerves are irritated. Cataleptic rigidity exhibits greater similarity in reference to its uniformity and duration, but is sufficiently distinguished by that enigmatical waxen flexibility, which, in the best-marked cases, enables the limbs to maintain even such positions as are opposed to the laws of gravity. Notwithstanding these remarkable differences, it may in particular cases be extremely difficult to decide at once whether rigidity or tetanus is present. This will especially occur where cadaveric rigidity sets in suddenly with a convulsion and extension of the limbs, or where actual tetanus passes immediately into cadaveric rigidity. The first occurs on the injection of various chemical agents into the arteries of the limbs of living animals. Here sudden rigidity ensues in the form of a tetanic spasm with extension. Flourens has suffered himself to be deceived, and calls this tetanic rigidity (*Comptes Rendus*, Jan. 13, 1851). Numerous investigations instituted by the author on this subject place it beyond doubt that we have to do, not with tetanus, but with real muscular rigidity, presenting all the essential characters of cadaveric rigidity. Sommer and Clemens (*Deutsche Klinik*, 1850, p. 512) saw, after rheumatic tetanus, the spasm of the muscles of the jaw, neck, and back pass immediately into cadaveric rigidity. But in general a stage of muscular relaxation appears to intervene between tetanus and the latter. "In tetanus produced by poisoning with strychnia, with the exception of Engel, who states the immediate passage into cadaveric rigidity as a rule, neither I, nor numerous other observers, so far as I know, could perceive anything of the kind." In poisoning with coal gas, too, rigidity seems capable of being developed directly from spasms with extension. The question will hence suggest itself—Can we from the nature of the rigidity alone determine the moment in which apparent death ends, and real death commences? Here, too, it seems

to me, the behaviour of the limbs in respect to forcible flexion may give decisive information, if general immobility of the joints be present. But where immobility of the lower jaw alone exists, it may under certain circumstances be difficult forcibly to overcome the closing of the jaw, whether it be a phenomenon of death or of disease, in which case the question can be decided only by an examination of other organs, particularly of the heart.

We have consequently obtained, as the fifth point, limiting the value of cadaveric stiffness, the difficulty which may exist, in particular cases, of deciding, solely from the nature of this condition, whether tetanic or cadaveric rigidity be present.

Nevertheless, cadaveric rigidity is, in the great majority of cases, an important and trustworthy sign of death, provided only that due regard be had to the possible sources of error. Its presence is almost always easily ascertained, and its characters are usually sufficiently well marked. But especially is it among the known signs of death, prior to the occurrence of putrefaction,—the most important of all those on which, for want of exercise or through dulness of hearing, a positive opinion as to the presence or absence of the sounds of the heart is to be based.

Lastly, a word as to a much-quoted sign of distinction between cadaveric rigidity and the so-called “rigidity from frost” (Froststarre); I mean the crepitating sound arising on motion of the parts, from the breaking of the particles of ice. We know that in death from frost the contractile parts become rigid before their water freezes; and when the latter thaws again, the rigidity continues for some time (Brücke). A frozen limb may, accordingly, at the same time present cadaveric rigidity; but a limb may also freeze, the rigidity of which has long since passed away, and in which decomposition has already begun. In both cases the crepitating sound will arise, and its presence proves only that watery particles are frozen, but not that neither cadaveric rigidity nor any of these various conditions of muscular stiffness are present. Another question is, whether this crepitating sound is a certain mark of the occurrence of death by frost.

On the circumstances which influence the duration and degree of cadaveric rigidity.—In order to answer the second and third of the questions stated above—“What data does cadaveric rigidity supply for calculating the time which has elapsed since death?” and—“What inferences can be drawn from its degree and duration as to the constitution, disease, and mode of death of the deceased?”—we must first make ourselves acquainted with the laws which govern the period of its occurrence and cessation, as well as its intensity.

First of all it is necessary to determine the very shortest and longest limit of the occurrence and cessation of natural cadaveric rigidity in the human subject. On this subject writers differ widely, and fresh investigations are much required. Sommer, who bases his statements on two hundred very careful observations, never saw rigidity set in, excluding the case of the direct passage of tetanic into cadaveric

rigidity, earlier than ten minutes, nor later than seven hours after the cessation of respiration. He, therefore, throws doubt on Nysten's statement that he has observed it not to commence in athletic men who lost blood to exhaustion, until sixteen or eighteen hours after death. In these cases, says Nysten, the rigidity attained an extraordinary intensity, which continued for from thirty-six to forty hours, and then gradually diminished, but did not wholly disappear until six or seven days after death. Probably it is safer not to receive Sommer's limitation in its full extent, but rather to admit the possibility that, in particular cases, where the muscular system is powerful, and death sudden, rigidity may not occur for sixteen or seventeen hours. In dogs, rabbits, and cats, the author never saw it set in later than four hours; most usually it commenced in one or two hours after death.

The shortest period at which cadaveric rigidity may completely pass off is under ten hours after death; it is very difficult to decide what is the latest time at which this may occur. Rigidity of the voluntary muscles may, under certain circumstances, continue fourteen days and longer; but the laws which govern that of the heart and smooth muscles are different. The several parts of the heart die at very different times. The irritability of the left ventricle is extinguished before that of any of the other muscular organs of the body. The author passes in review the several structures of the body, in reference to the duration of irritability in each; and next proceeds to the consideration of the laws known to us in reference to the circumstances which influence the duration and intensity of cadaveric rigidity.

I. Nysten has propounded the law, that *rigidity continues longer in proportion as it has set in later after death*, but the exceptions to this rule are very numerous. Bodies sunk in cold water very soon stiffen, and retain their rigidity long, because this medium is a better conductor of heat, but is less favourable to decomposition than atmospheric air.

II. Another law of Nysten's sets forth that *the intensity and duration of cadaveric rigidity is always in direct proportion to the strength and integrity of the muscles of the body*; from which will follow, as corollaries:—1. *That, ceteris paribus, rigidity is weaker, and of shorter duration, in infants and children than in adults*; it also generally sets in earlier in the former than in the latter. This statement has been, by Sommer, extended to include the old, which is not, however, fully borne out by an examination of Albers' Tables. 2. *The more quickly an individual dies, the stronger and more persistent is, under like circumstances, the rigidity, and the later does it usually set in.* 3. *The more the muscular nutrition has been injured by the nature of the preceding illness, the weaker and more transitory is the rigidity, and the earlier does it commence.* Sommer, who completely corroborates the latter two laws, so far as the influence of the duration of the act of dying, and of the nature of the disease, on the intensity and duration of cadaveric rigidity, is concerned, denies that the time of its occur-

rence is, in an equally striking and constant manner, dependent thereon, and he supports his statement with numerous examples.

III. *The more powerfully an agent depresses the vital energy of the muscular fibre, the more quickly does it become rigid.*

A. Above all things, the presence of free oxygen in the muscular fluid is necessary to the muscle for the maintenance of its vital properties. Without this, it cannot breathe, renew itself, or fulfil any physiological function. With the cessation of muscular respiration, with the consumption of the oxygen, and supersaturation with carbonic acid, cadaveric rigidity sets in. The more rapidly, therefore, the oxygen is withdrawn, the more quickly does the muscular fibre, other things being equal, fall into this condition. Hence are explained the following facts:—

1. Those animals (as birds) which, during life, consume most oxygen, fall earliest; those which (as the Amphibia) use least, fall latest into the state of cadaveric rigidity. The frog, in general, does not become rigid until from one to three days after death.

2. Muscle stiffens later in an atmosphere of oxygen, and earlier in one of carbonic acid.

3. As in the living animal the access of oxygen to the muscle takes place through the circulation and the arterial blood, cutting off the latter by tying the afferent vessels, or its exclusion in consequence of the obstruction of the vessels by the formation of a plug, must be followed by cadaveric rigidity of the corresponding groups of muscles. The experiments of Brown-Séquard, and Stannius, have further shown that the rigidity may be again resolved by the restoration of the circulation, provided too long a time shall not have been allowed to elapse. The injection of defibrinated blood restored the mobility of an arm in thirteen hours after execution; but failed to reproduce that of the leg of the corpse when employed after the lapse of twenty-seven hours.

4. The more a muscle has been stretched in action, so much the more quickly does it stiffen; for the more it acts, the greater metamorphosis does it undergo, and it must also take up more oxygen and excrete correspondingly larger quantities of carbonic acid. Hence is explained the problem why animals hunted to death so rapidly become rigid, and if the rigidity is at the same time weak, and does not last long, this depends on Nysten's second law, as, of course, the integrity of the muscle must, under such circumstances, have suffered considerably. Therefore, also, according to Mitscherlich's experiments, the irritability of muscles is extinguished sooner, in proportion as their energy is exhausted by spasms; and according to Brücke, cadaveric rigidity sets in with remarkable rapidity in poisoning by strychnia; and Leblanc and Faivre have observed the same in tetanus produced by veratria. As already mentioned, Sommer and Clemens saw, in the human subject, tetanic pass immediately into cadaveric rigidity. But whence it proceeds, that, as Brücke states, the rigidity after poisoning by strychnia, notwithstanding its earlier access, continues very long, while we should suppose "that the

more active muscular metamorphosis excited by the tetanus must also occasion an earlier supervention of decomposition," appears to require further explanation.

5. Lastly, Brown-Séquard has shown, that on dividing the sympathetic nerve on one side of the neck, the corresponding half of the face retains its irritability much longer, is later in becoming rigid, and also in entering into putrefaction, than the other side, the sympathetic nerve of which continues uninjured. But if, on the contrary, the sympathetic nerve of one side be galvanized for a long time, its irritability is sooner lost, and rigidity and decomposition set in earlier. Perhaps these circumstances depend on the stasis caused by the division of the nerve, while the galvanization produces a constriction of the vessels and a diminution of the mass of the blood.

B. Muscle requires for the maintenance of its life a certain medium temperature, which, however, varies in cold and warm-blooded animals. If it be exposed for a certain time to a temperature differing in either direction from the normal, it loses its irritability and becomes rigid, as has already long been known. This change takes place more rapidly in proportion as the difference between the warmth of the muscle and that of the surrounding medium is more considerable. Sommer has certainly shown, that rigidity usually sets in in the human subject not only before complete cooling, but that it also may be developed prior to the sinking of the natural warmth, and even while a morbidly elevated temperature exists; the old opinion, according to which the ordinary cadaveric rigidity was thought to be a consequence of cooling, is therefore for ever refuted. It must, however, be understood that the effect of a rapid external abstraction, or of an artificial elevation of the heat of the body in hastening the occurrence of this phenomenon, is not hereby excluded. Sommer's merit consists in having shown that it requires more considerable difference between the temperature of the body and that of the surrounding medium, than had been before the publication of his researches supposed, in order to obtain striking results.

Thus it had been maintained by Nysten that bodies which have remained in bed stiffen much later than those which are exposed to the open air. Sommer has made many experiments on this point without having attained to any decided result. It appeared to him that, other things being equal, bodies became rigid as quickly in an atmosphere of from 59° to $63^{\circ}5'$ as in one of from 77° to $81^{\circ}5'$. Finally, he convinced himself of the incorrectness of Güntz' assertion, that the bodies of infants did not stiffen in tepid baths (from 65.75 to 99.5). Richot had previously made the same statement, and Orfila also said that a tepid bath retards the occurrence of rigidity. Sommer placed two infants, which had died asphyxiated, in tepid baths of from 90.5 to 99.5 immediately after birth, and left them there.

* The thermometric readings throughout this paper have been reduced from Reaumur's to Fahrenheit's scale.—TRANS.

In between three and four hours rigidity was developed, and it attained its height in six hours. In eleven hours the rigidity of the lower jaw had again disappeared. Güntz and Sommer agree, however, in stating that a temperature of from 32° to 59° exercises a greater influence, particularly on the duration and intensity of the rigidity; thus the bodies of strong persons may continue rigid for eight or ten days at a temperature of from $36^{\circ}5$ to $45^{\circ}5$, while the last trace of rigidity disappears from them in from four to six days when they are exposed to one of from $65^{\circ}75^{\circ}$ to 86° . On the other hand, according to experiments instituted by Güntz and Sommer on the bodies of children, the rigidity disappears more quickly the higher the temperature is elevated above $99^{\circ}5$.

It has been supposed that injections with cold water have a considerable effect in hastening the occurrence of rigidity, but the author, to his surprise, did not obtain any striking result of this nature by throwing in either cold or hot water.

C. It is only when it contains a certain amount of water that a muscle can exercise its function. *Drying causes rigidity*. Observers are agreed on this point, that a muscle during the state of rigidity is drier than before and after. Dropsical bodies fall early into rigidity, though this is but slight, and does not last long. Nysten and Sommer observed that a dropsical limb maintains its rigidity more energetically and longer when the œdema is confined to the subcutaneous areolar tissue, than when its muscles also are infiltrated with fluid.

The question, whether an increased dryness is the cause of rigidity has therefore some plausibility, and, in fact, Krause sees in the former one of the most important causal elements of the latter. The author, however, believes Krause's view to be untenable: first, because rigidity sets in indifferently whether the body is surrounded by air or water, and also occurs after the copious injection of water. Secondly, because in Mammalia and frogs he succeeded, notwithstanding that large injections of water had been thrown in, in immediately producing, by the injection of a few drops of chloroform or some drachms of ether, the most intense rigidity in the infiltrated limbs. Lastly, he saw rigidity supervene, after the arteries were tied, in those regions of the limb which were without doubt still abundantly moistened from above.

D. The living muscle is placed in certain mechanical conditions of tension, which cannot be altered beyond a particular degree without danger of injuring its vital properties. Dubois proved that muscles excessively and too long extended, or strongly compressed, are soon injured in their functions, and become rigid. Krause showed that, as has already been mentioned, a muscle can in general attain the state of ordinary cadaveric rigidity, only when it is exposed to a certain degree of tension. Perhaps it is to this that the observations of the absence of rigidity in broken limbs is to be referred.

E. A muscle is, so long as it breathes and lives, constantly sur-

rounded by electric streams, and, except that the direction of the current is changed, indifferently whether it be in a state of rest or of contraction. Essential, however, as the electric fluid appears to be to the life of the muscle, it may become dangerous when it acts in the form of violent discharges, or rapidly traverses the muscle as a strong and especially as an intermittent current.

The old popular belief, that individuals struck dead by lightning do not become rigid, and that they rapidly putrefy, seems not to have obtained at all times and among all people. In Plutarch we find it asserted that such persons are very long without falling into a state of decomposition, while Seneca declares the contrary:—"Fulmine icti inter paucos dies verminant!" The rapid transition into a state of decomposition of bodies killed by lightning has undoubtedly given rise to the statement that they do not stiffen. Experiment, however, shows that animals killed by strong electric shocks become rigid, and the statement just alluded to is refuted by observation of the bodies of men killed by lightning. The experiments of Brown-Séquard prove that an electric current has the effect of hastening the occurrence both of rigidity and of putrefaction; and it has been found that an intermittent stream induces stiffness more rapidly than one which is continuous.

F. Numerous chemical agents possess the power of instantaneously producing rigidity of the muscular structure, or of greatly hastening the supervention of this state. Of these, some, as ether and chloroform, are capable, when injected into the arteries, of producing stiffness only before natural cadaveric rigidity has set in, but are incapable of reproducing it when it has passed off; while others, as water of caustic potash, acetic acid, and dilute hydrochloric acid, excite a certain degree of rigidity in muscle which has again become relaxed and is in process of putrefaction. The agents which the author found in the smallest quantity to occasion the greatest degree of rigidity, were oil of mustard and chloroform; and he also observed that the rigidity produced by chemical agents in the limbs of the bodies of animals lasted in general much longer than that which took place spontaneously.

There is no doubt that cadaveric rigidity furnishes data for calculating the time which has elapsed since the decease of an individual, though our knowledge is insufficient to afford more than approximate results. The author sketches some imaginary cases in illustration of the extent to which inferences may be drawn as to this point, from the circumstances in which bodies are found, and of the mode of reasoning from the premises so supplied.

Cadaveric rigidity may be useful in some cases in determining the priority of death.—This part of the subject is exemplified in the same manner. Dr. Kussmaul considers it to be very problematical, whether the period at which rigidity supervenes and goes off, or its intensity, shall ever justify conclusions as to the previous illness and mode of death,—as for example, poisoning with mushrooms, or with strychnia, the effects of lightning, &c.

The author lastly inquires to what errors cadaveric rigidity may give rise in the examination of remains. It is to be remembered that the limbs usually stiffen in the position they have last received. That the corpse of a person shot should be found with the fingers closely grasping a pistol, is no proof of suicide; for if this position be given them in the stage of relaxation, they will in that of rigidity embrace the object so closely, that it will be difficult to disengage the latter. The transient expression of the countenance produced by passion and emotion, anger, pain, terror, &c., and essentially due to contraction of the facial muscles, must disappear after death, because these immediately relax, and some of them subsequently contract when rigidity comes on. It is, therefore, quite inadmissible to attempt to infer the final state of the mind during life from the physiognomy of the corpse. No conclusion can be drawn from the width of the pupils in death, as to the diameter they presented in the latest period of life. Rigidity of the heart exposes us to the danger of looking upon this organ as diminished, its muscle as condensed, and the cavity of the left ventricle as narrower than in the normal state. Probably the entire theory of concentric hypertrophy of the heart depends upon an error, rigidity of the heart having been looked upon as a pathological condition. On the other hand, an inexperienced observer has often allowed himself to be deceived by the existence of relaxation of the heart into the diagnosis of a passive hypertrophy. It is evident that paralysis of the heart is not to be inferred from the post-mortem appearances, especially from flaccidity of the muscular structure. In order to distinguish the hardness of an hypertrophied from the apparent firmness of a rigid heart, it will be advisable to cut out a portion of it, and to try how far the stiffness can be lessened or removed by pulling and bending. The ruffled condition, frequently exhibited by the surface of the spleen, is the consequence of constriction of its vessels, and perhaps, also, of shortening of its contractible fibres. Whether the contracted state of the stomach, the wrinkled and "glandulo"-varicose condition of its mucous membrane, so often met with in early dissections, depends on rigidity of the muscular coat, cannot be accurately decided. We should, however, beware of considering them as pathological phenomena.

Lastly, the greater ease with which a rigid muscle may be torn, should be kept in view. The stronger the individual was, and the more intense the rigidity is, the more likely are the muscles to be lacerated if roughly handled. The so-called spontaneous rupture of the recti muscles of the abdomen is attributable to another circumstance, namely, to increased fragility in consequence of inflammation of the muscles.—*Vierteljahrsschrift für die praktische Heilkunde*, 1856. Band 2, p. 67.

Cases illustrative of the beneficially Antispasmodic and Anæsthetic Effects of Chloroform. By Prof. P. H. MALMSTEN of Stockholm.

CASE I.—A girl, aged nearly seven years, herself a healthy and sprightly child, but whose brothers and sisters in general showed a tendency to sanguineous congestion of the head and convulsions (one sister had died of acute hydrocephalus), complained on the 22nd of last March, having the day before probably taken cold, of headach fixed in the left temple; she was depressed, but was free from fever; the bowels had not acted during the preceding twenty-four hours. Castor oil was given at noon, but had no effect; still, as the child was more lively towards evening, her parents did not think it necessary to take any further steps. During the night her sleep was disturbed, and she complained of headach. At 10 o'clock of the forenoon of the 23rd a violent attack of convulsions set in. Physician-in-ordinary (Lifmedicus) Ahlberg, who shortly after accidentally saw her, found her suffering from incessant convulsions, with violent congestion of the head; the eyes were distorted, the conjunctiva, especially of the left eye, was injected, the pupils were contracted. A hot pediluvium was immediately ordered, together with a stimulating enema, the application of ice to the head and five leeches behind the ears. Dr. Ahlberg also prescribed a powder of calomel and jalap. On my arrival at a little before 12 o'clock I found the child lying in a state of constant insensibility, with severe convulsions, distorted eyes, and much heat of head. The powder was immediately administered, the hot pediluvium was again employed, but as no improvement was apparent, the little patient was placed in a warm full bath, while the bladder of ice was kept on the head. It was not until 1½ o'clock, after she had been in the bath for more than a quarter of an hour, and consequently when the convulsions had continued for three hours and a half, that the latter ceased; she was then put to bed. Notwithstanding that the leeches bled considerably, the head was hot and the countenance flushed. The eyes were closed; the child was insensible; the skin was dry and hot; the pulse was quick. The application of ice to the head was continued; a purgative and stimulating lavement was administered, and three table-spoonfuls of compound infusion of senna were given internally. On my return at 4½ o'clock, the child, who had meantime been quiet, once more knew those about her and spoke some words to her parents, when suddenly an attack of violent convulsions again set in. The hot pediluvium was repeated, and musk was given internally. After the foot-bath, sinapisms were applied to the calves of the legs and to the epigastrium; but as no amelioration took place, the full warm bath was again had recourse to, the child being kept in it for twenty minutes. The convulsions, nevertheless, continued; the little patient was replaced in bed; her complexion became more and more livid; the convulsive spasms, which occupied almost the whole muscular system, began to predominate in the extremities of one side; the pulse was weak, and

not particularly frequent; bloody foam came from the mouth. Fearing that collapse and death might quickly supervene if the convulsions were not arrested, I determined to let the child inhale chloroform, which I had brought with me. Twenty drops of chloroform were put upon a handkerchief, which was held to the child's nose, at a distance of an inch and a half, with the rapid and surprising effect that the extremely violent convulsions were immediately diminished, and in less than a minute and a half had entirely ceased. When occasionally they appeared about to return, they were checked by causing the child again to inhale chloroform for a very short time. The patient now grew pale, and lay insensible, but calm and quiet; the pulse was steady, about 100. A copious alvine evacuation soon took place. The application of ice was continued, and two table-spoonfuls of compound infusion of senna were again given. Instead of administering mercury internally, I preferred, in order to bring the little patient as quickly as possible under the influence of that remedy, to employ inunctions of mercurial ointment in the groins, the inside of the thighs, and the axillæ; accordingly, half an ounce was rubbed in the evening. A moist warm poultice was applied over the stomach. The ensuing night was tolerably calm, except that the child several times ground her teeth. On the morning of the 24th she looked up and was tolerably collected, but did not answer when spoken to; her face was hot and red, notwithstanding that the bladders of ice were kept constantly applied. Two drachms of mercurial ointment were again rubbed in, and as a copious action of the bowels had taken place no medicine was given internally; she had milk and water for drink. In the evening of the same day the child began to speak and to exhibit full consciousness. Slight mercurial fetor of the breath was now perceptible. The little patient complained of constant pain in the left temple. Autenrieth's [tartar emetic] ointment was rubbed into the neck morning and evening, and every second day a gentle dose of compound infusion of senna was given to keep up an action of the bowels; no other internal medicine was administered. It was not until the seventh day of the child's illness—when her condition was gradually improving, while a very gentle mercurial action continued, and the ointment had brought out an eruption on the neck—that an attempt was made to leave off the application of ice to the head; but the face soon began to get red, the head became hot, and the child got restless, on which account it was continued for two days longer, and was not removed until the eighth day of the illness. A little nourishment was gradually and carefully given. On the 2nd of April the child was entirely free from fever; on the 3rd she could, although extremely weak and debilitated, play a little with her dolls. The improvement continued uninterruptedly, and at the end of fourteen days more the little patient was quite recovered.

The above case speaks so fully for itself, that any observations are really unnecessary. That the child would most probably have died, if we had not succeeded in quieting the convulsions by means of

the chloroform inhalations, I am convinced, as I had six years before seen a nearly similar case, in which the convulsions did not cease until the child fell into deep coma, which was soon followed by death. We must consider ourselves truly fortunate to have gained so efficacious a remedy for such desperate cases; for the first indication must still always be, to arrest the convulsions. Further, I feel myself called on to recommend that when the indications exist in infantile practice for the use of mercury as an alterative, the ointment should be employed externally, instead of giving the medicine internally, for the digestive organs are then not loaded, diarrhœa is avoided, and the child's strength is incomparably more husbanded, than when the remedy is given internally.

CASE II.—On the 9th of April I was called to a middle-aged man of strong constitution, who had in general enjoyed good health, but had, after some days' general indisposition, been attacked during the preceding night with violent headach, high fever, and tenderness over the whole body. The night had been sleepless. On my arrival at 10 o'clock in the forenoon, the patient complained incessantly of severe headach, great uneasiness and pain in every part of the body; the head was hot; the face was flushed; the eyes were intolerant of light; the pulse was hard, full, and frequent; and the skin was hot and dry. From the symptoms then present, and from the fact of the patient having been ill for some days, I apprehended the commencement of a nervous fever. Ten grains of calomel were given in the morning, and ice was applied to the head. In the evening of the same day, after some alvine evacuations had taken place, he was somewhat more tranquil, but the headach was violent, the fever and uneasiness continued. The application of ice was continued, and a solution of phosphoric acid was given internally. On the 10th the patient had a more tolerable night, and the headach and fever were diminished. The same treatment was continued. On the 11th the patient was somewhat better; the headach and fever were inconsiderable, but there was great debility. On the forenoon of the 12th he complained of weight in the head, and felt particularly weak and depressed; there was not much fever. At 2½ o'clock in the day he was attacked with severe headach, which gradually increased, and when I visited him at about 4½ o'clock he complained of an extremely distressing pain in the right ear, accompanied by sensations of cutting and rending, extending inwards over the right side of the head, and over the left temple. The head was hot, the countenance was flushed, and the temporal arteries were beating strongly. Two bladders of ice were now directed to be applied to the head, ten leeches to be put behind the left ear, and four table-spoonfuls of compound infusion of senna to be given internally. I returned at 7 o'clock; the pain in the ear was now still more distressing, and the sufferer, who was in general extremely patient, complained very much of the tearing sensation in the ear; the pulse was rapid, small, and hard. On inspection, nothing wrong could be discovered in the external parts of the ear. I di-

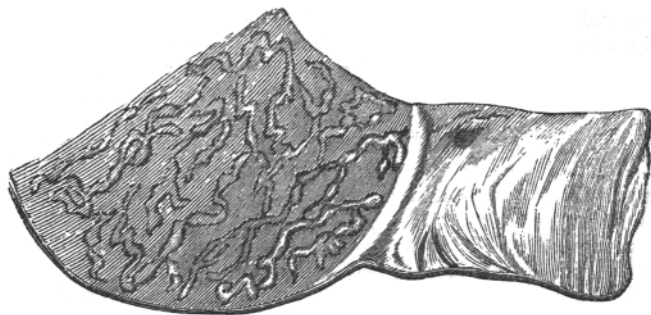
rected morphia dissolved in warm oil to be dropped into the ear, and as the leeches had recently begun to bleed, I hoped some relief from his sufferings should have been obtained. On my return at 9½ o'clock in the evening I found the patient quite distracted with the pain in the ear, which produced agitation throughout the whole body, and sometimes a tendency to vomit. The leeches had drawn copiously; the head was cooler; the bowels had acted, but no relief had been obtained; on the contrary, the pain had increased. The patient declared that if he did not quickly get some ease, he should probably become deranged. In this desperate state of things I determined to try the effect of dropping chloroform into the ear. About twelve drops were introduced at once; the man immediately became almost delirious in consequence of the violence of the pains in the ear, but within a minute after, the latter abated, and in five minutes perfect quiet and freedom from pain ensued, only a throbbing and heat remained in the ear. A warm poultice was applied over it; the ice was continued to the head, and blue pill was given at intervals during the night. On the 13th the patient, who had towards morning some hours' rest, was very weak; the fever was inconsiderable; some feeling of weight in the head alone remained; the pain in the ear had ceased. The blue pill was continued during the day. The night of the 14th the patient slept quietly, and in the morning was free from fever, but weak. At 2½ o'clock I got a message that the pain had returned in the right ear as severely as on the 12th. I sent directions to have the ice applied immediately, and on my arrival at 4 o'clock I found the patient complaining of violent pain and cutting sensations in the ear, precisely as he had done two days previously. I now without delay dropped the chloroform into the ear, with the same result as before, namely, at first, and immediately after it was dropped in, intolerable pain, which drove the patient for an instant quite distracted; but shortly after, perfect quiet and removal of all pain. No chill or shivering had preceded either attack, nor had any perspiration followed it; but the disease was evidently a masked intermittent fever, which might well be called malignant (*perniciosa*). On the 15th, accordingly, twenty grains of quina were administered. The patient, who had subsequently no return of the attacks, recovered, however, but slowly, and had for long a feeling of weight in the head, and, as it were, of a band over the forehead: in fact, three weeks elapsed before he could consider himself at all restored to health.—*Hygiea*, May, 1855, p. 372.

Two Cases of unusual Abnormalities in the Valve of the Pylorus. By
PROFESSOR A. RETZIUS.

EXAMINING last year at autopsies a great number of stomachs, with a view to study the form of this organ, I met two, as they appeared to me, remarkable varieties of the valve of the pylorus, which,

so far as I am aware, have not heretofore attracted the attention of pathologists. In both cases I am, however, ignorant of the condition of the subject during life.

1. *Obliteration of the Valve of the Pylorus.*—In a stomach which was rather small in circumference, and was taken from a middle-aged woman, with respect to whom I have no further information, the valve of the pylorus was found to have nearly disappeared. The pylorus itself stood open; of the valve only a low circular border, scarcely a line in height, was found; the muscular rings in it were proportionally very slight. I append a figure of the pyloric portion of this stomach, representing the pylorus and a part of the duodenum, half the natural size. The valve is formed only of a low border.

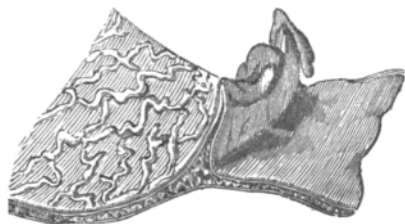


The preparation was taken shortly after death, before the cadaveric rigidity had passed away. I consider that this pyloric apparatus cannot have been in a condition completely to close the stomach. If this was the case, what disturbance must not such a circumstance have produced in the process of digestion in the stomach? It is evident that when the pylorus is only imperfectly shut, the food introduced into the stomach must, in many instances, be less exposed to the action of the gastric juice than when the organ is entirely closed during the first part of the process of digestion. This must especially be the case when the food is thin.

I have no reason to believe that this condition was congenital. What produced it? I know not, and I do not venture in this case even to offer a conjecture. The coats of the stomach were, so far as I could see, sound, without any trace of ulcers, scars, or other anomalies.

2. *Unnatural Prolongation of the Pyloric Valve.*—This stomach was likewise that of a female, older, however, than the preceding subject; neither was I able to procure any information as to her state during life. The organ was rather small. The sacculus cæcus, and also the antrum of the pylorus, were but slightly developed;

the entire stomach was long and slender. The valve of the pylorus itself projected as a somewhat conical beak into the antrum duodeni^a. The length of the valve from the base was nearly an inch. Only immediately adjoining the base did it contain a ring of muscular fibres; otherwise it consisted of a fold of mucous membrane, containing submucous connective tissue. The surface of the mucous membrane exhibited nothing abnormal.



This was the first time I had observed such a case; nor can I recollect having seen a description of anything similar. It appears to me to be evident that neither could this pyloric valve properly discharge its function. Nor do I believe that in this case, any more than in the preceding, the pylorus could be perfectly closed during the primary digestion of the food. Probably this had a disturbing influence on the first part of the duodenum, which is free from valvulæ (the antrum duodeni). I imagine that if this valve, in a case of severe vomiting, should have come to be turned in upon the stomach, it must have produced a constant irritation of the sensitive antrum pylori. Equally little can I, in this instance, form any idea of the origin of the affection; I believe, however, that, as in the preceding case, it was not congenital, but acquired.

We have hitherto in general directed quite too little attention to both the orifices of the stomach: the cardia, and pylorus. As concerns the cardia, it may as well be looked upon as belonging to the œsophagus as to the stomach. The closing of the upper orifice, too, is most closely connected with that of the œsophagus. All the circular muscular layers of the gullet act as a sphincter throughout the whole course of the œsophagus, down to the cardia, and may, therefore, be considered as a broad sphincter of the upper orifice. Without some such considerable and powerful closing apparatus, the slightest pressure on a stomach filled with fluid substance would cause the latter to rise into the throat and mouth, and perhaps even into the nose. It is therefore evident that this circular muscular apparatus in the œsophagus is constantly in a contracted condition, like the sphincters

^a A name given by the author to the first part of the duodenum, which wants the valvulæ conniventes of Kerkringius.

of the anus, and does not yield until it is distended by a *vis a tergo*^a. In the same manner it appears to me that a sound pylorus ought to be habitually closed, and to be opened only occasionally, when anything is to be expelled into the duodenum and the rest of the intestinal canal. This closing must chiefly depend on the muscular ring, which has been called the sphincter of the pylorus; but it would seem that here, with the sphincter, a valve is also necessary, as if to fill the opening during contraction, thus sparing the sphincter the necessity of wholly closing its ring. Moreover, this valve is intended to hinder regurgitation from the duodenum into the stomach when the pylorus is opened. Accordingly, when this organ is so deficient as it was in the first case, such a regurgitation from the duodenum of bile, &c., must have taken place just as the pultaceous contents of the stomach may be supposed to have run down prematurely into the duodenum, before they were completely acted on by the gastric juice. I imagine, in fact, that when the valve is so obliterated as was the case in this instance, it must be necessary that the muscular ring itself should almost fully contract in order to close the pylorus. But the pyloric portion of the stomach is usually much thicker than the remaining part of the walls of that organ, and often possesses a considerable degree of rigidity. This thickness and rigidity of the pylorus must in no small degree impede the perfect closing of the sphincter, in the completion of which, therefore, the valve with its mass assists by filling up the deficiency.

The pylorus must necessarily be provided with a peculiar, so to speak, sensitive excitability, in virtue of which it obeys the summons to open itself, when portions of the contents of the stomach are to pass through. We must, therefore, suppose that in a sound stomach, this nervous excitability must be in well regulated order, always, as it were, on the watch to receive impressions, and to communicate these to the nervous centres, whence the impulses proceed to the muscular parts, effecting the opening of this portal. Impairment of this excitability, or an interruption of its conduction, must in many cases destroy the function of the pylorus. This suggests the idea that the motor functions of the pylorus may be imperfect or disturbed. This may depend on defects in the nervous centres, in the conducting media, or in the muscular ring itself. All these circumstances will require to be more closely studied, to enable us to recognise and advantageously deal with them. This

^a John Müller, in his Physiology, has directed particular attention to the peculiarity of a sphincter muscle, that, unlike most other muscles, it is always contracted as if excited thereto through nervous influence, and he compares this property to a constant rigid cramp. What we usually denominate cramp in the œsophagus is a phenomenon of quite a different nature from what is elsewhere called cramp. That is to say, it must consist of an irregular simultaneous action of the longitudinal fibres and the various rings of circular fibres, which is rather to be compared to a partial and imperfect paralysis; in other words, to restricted action of the same muscular apparatus, the result of morbid deposits about it, or of mechanical obstruction in the neighbourhood of the œsophagus, in or near the posterior mediastinum.

appears difficult, but I believe that it is not impossible. We have many reasons to suppose that the stomach has a very regular action, that it is capable, by means of its ingenious muscular apparatus, of dividing itself into several compartments, and of disposing certain portions in certain of these cells, &c. This must especially be the case with the permanent division I have so often spoken of, and so constantly occurring, to which the name of the antrum pylori has been given, on which undoubtedly all excitants of vomiting, and specially emetic remedies, must exercise a particular influence.—*Hygiea*, October, 1854, p. 588.

On the Effect of Sleep on the Excretions. By BECKER.

THE author's experiments showed, that during sleep the quantity of the urine, of the water, of the solid matters, of the urea, of the volatile salts, of the chloride of sodium, of the extractive matter, and of the earthy phosphates, was increased. On the other hand, the quantity of the fixed salts, and of the sulphuric acid in combination with potash, remained unaltered, while the urates and alkaline phosphates were considerably diminished. The loss of weight of the body, which was always much greater during sleep, was attributed to the increased excretion of urine. The insensible perspiration would appear, from the inconsiderable variations which the different experiments exhibited, not to be materially affected by sleep or waking; still, if it be true that during sleep the quantity of carbonic acid exhaled is diminished, the insensible perspiration must undergo a corresponding increase. The author explains the established diminution of phosphoric acid as follows:—This acid is found in the muscular fluid combined with potash, and in the brain as oleo-phosphoric acid. But its amount in the urine does not at all correspond, as one should expect, to the urea abundantly separated through the rapid metamorphosis of the muscular substance, but appears, on the contrary, often diminished in proportion to it (the urea); consequently we cannot connect the diminished excretion of phosphoric acid with the metamorphosis of the muscles, or muscular fluid, but only with the brain. But of the brain itself, only that part which carries on the psychical function can, properly speaking, rest during sleep, as in the vegetative functions of the body scarcely any cessation is demonstrable. The author, therefore, believes that under otherwise equal conditions during sleep the metamorphosis of the part of the brain (and perhaps also of the spinal marrow), which performs the psychical functions, is diminished, while that of the other organs, so far as their products of metamorphosis leave the body as constituents of the urine, is increased, and then the renovation, especially that of the brain, is effected.—*Vierteljahrsschrift für die praktische Heilkunde*, Band 50, 1856, *Analekten*, p. 5.