CHLOROFORM:

HOW SHALL WE ENSURE SAFETY IN ITS ADMINISTRATION?

BY

PATRICK BLACK, M.D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS,

ASSISTANT-PHYSICIAN TO, AND LECTURER ON MEDICAL JURISPRUDENCE AT, ST. BARTHOLOMEW'S HOSPITAL.

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ON

CHLOROFORM.

THE constant use of Chloroform in medical and surgical practice, renders it of the greatest importance, not only that the theory of its successful action should be rightly understood, but also that we should have not less distinct ideas concerning the true nature of the danger which is incidental to its administration.

Without this latter knowledge,—which, it must be observed, is quite distinct from the former, and does not necessarily flow from it,—we shall be constantly exposed to the recurrence of those appalling accidents which have caused many, and not without some reason, to regard chloroform as a very questionable boon, which would lead others to reject it altogether, and have inspired all with the gravest anxiety lest that fatal casualty which has occurred in the hands of others of undoubted competence and ability, may at some time become unexpectedly chargeable to themselves.

On the present occasion it is not my intention to enter on the consideration of the theory of Anæsthesia,—a branch of the subject on which my views are quite in accordance with those which have been ably expounded by Dr. Snow and others, and are generally adopted by the profession ; but to confine my remarks to the question of the exact source of danger in the administration of chloroform,—to the

physiological views which are intimately associated with that inquiry,—and to the practical conclusions which are forced upon us by the concurrent evidence of experiments on animals, by the analysis of the best-recorded fatal cases, and by the suggestions of physiological reasoning.

In treating this subject in the manner proposed, I shall have to make occasional reference to the writings of Dr. Snow, and to quote his observations as in part the foundations of my argument; and whilst I shall be constrained to differ from him, and to reject the reasoning which he offers to our acceptance on the cause of death by chloroform, I would gladly bear testimony to the great ability with which he has investigated the whole subject, and freely acknowledge my inability to offer anything in addition to the precautional measures he has given us in order to ensure safety in its administration.

It will appear from this, that the difference between us is one rather of theory than of practice; that we adopt the same practical conclusion, but on different grounds; our inferences are the same,—not so, however, our intermediate reasoning.

It might here be suggested: "If there is this entire concurrence between you as to practice, what need is there for further investigation; for practice is, after all, the end and object of your speculations?" We would reply to the question even upon that issue, and will endeavour to show that security against accidents cannot be based on any practical rules, however excellent, which do not recognise a sound theory. We might even show that practical rules have miscarried on two occasions in the hands of the very ablest operators; and they have miscarried for this reason, —that the symptoms of danger, though present, have not been understood; they have consequently been disregarded, because they were not appreciated.

The cause of death by chloroform, according to Dr. Snow, consists in "paralysis of the heart," from the vapour of chloroform having been inhaled in too concentrated a form. He thus expresses himself: "In some cases sufficient chloroform is absorbed to arrest the action of the heart by its own influence:" in another place, "the heart is disabled by the direct action of the chloroform:" in a case where death took place in about half a minute, he observes "that the air the patient breathed must have been charged with the vapour of chloroform to a dangerous extent, and that the blood could not have been uniformly impregnated with the narcotic in so short a time as half a minute: the heart would therefore be liable to be paralysed by its direct action."

It is unnecessary that I should multiply such quotations, the views of Dr. Snow being sufficiently manifested by those already cited, which imply that the vapour of chloroform may be inhaled in too concentrated a form, and may thus prove fatal by paralysing the heart.

There is something in the evidence which may be collected from various sources on this point, which at first sight may seem to favour this explanation; but perhaps the principal ground on which it rests is to be found in the suddenness with which the alarming symptoms have come on.

There is no circumstance which is more remarkable in all the recorded cases of death by chloroform, than the extraordinary rapidity with which the fatal symptoms have supervened. The pulse, it is said, has suddenly ceased to beat without any previous warning. The remark has been made that "chloroform has been absorbed into the blood with such rapidity that there has been no time to watch its effects." In one case the time has been half a minute; in another, seventy-two seconds; in a third, one minute, or less; in a fourth the fatal event is thus described: "she

coughed a little, and then gave a few convulsive movements: when these subsided, the necessary incisions were made: scarcely a drop of blood escaped, showing that death had already taken place." In another case the time consumed in the operation was too short to be estimated by minutes, in the usual way; the description of it is the following:— "Scarcely had she taken several inspirations, when she put her hand on the handkerchief to withdraw it, and cried with a plaintive voice, 'I choke.' Immediately the face became pale, the breathing embarrassed, and she foamed at the mouth; the operation—a trivial one—was performed, but the patient showed no signs of life, and the operator believed that she was dead when he began it."

These cases are not selected for the purpose of proving a disputed point, but are the first recorded ones; the remaining cases are to the same effect, and show with equal distinctness the sudden and unexpected manner in which the fatal symptoms have supervened.

But after admitting these facts—which, indeed, cannot be questioned—are we justified in drawing the conclusion that chloroform proves fatal by paralysing the heart? We do not think so. Paralysis of the heart would be death by syncope, and the symptoms before death, as well as the condition of the organs disclosed by post-mortem examination, would be in accordance with such a view of the case.

We shall revert to these points; but in the meantime we would invite attention to the distinction between two totally different classes of phenomena, which have sometimes been confounded,—viz. "*asphyxia*" and "*narcotism;*" as it appears that it is in a right understanding of these essentially different conditions, that the solution of the difficulty before us is to be found.

An animal may be either asphyxiated or narcotised by the very same agent, according to the respective circum-

stances in which he is exposed to its operation. For instance, an animal may be placed in a vat over a fermenting mass, in which the atmosphere around consists either entirely of carbonic acid, or of air which is very highly charged with it. What are the phenomena which would be observed in such a condition? The animal would immediately fall down; struggling or convulsions would ensue, speedily followed by insensibility; and death would take place within two or three minutes—perhaps within one minute—from the commencement of exposure. An animal so killed would be suffocated or asphyxiated: it would be placed in a condition in which the expansion and movements of the chest would be impossible: the circulation would be forcibly arrested, and death would in consequence immediately ensue. But let an animal be placed in air containing a certain, but still a respirable, quantity of carbonic acid, the phenomena which would then be manifested would be totally different: respiratory movements would take place without interruption; the circulation would proceed unaffected,—at least, not violently affected; delirium might ensue, to be followed by sleep, becoming gradually deeper, and at length comatose; the circulation would become more sluggish, the temperature of the body sink, and death would ensue without any of those symptoms of violence which are characteristic of asphyxia. In the former case death would take place by asphyxia; in the latter, by narcotism.

I am aware that the view which I have thus advanced is not that which is always given by physiologists. They would perhaps say that in the one case the animal is suddenly or *rapidly* asphyxiated, and that in the other he is *slowly* asphyxiated,—that it is only a question of degree, and not of essentially different physiological manifestations. I cannot accept this conclusion: the phenomena in question

involve an essential difference most marked in their character, and of the greatest importance. In the one case the air is respirable, in the other it is not respirable: in one case the movements of the chest are regular and unimpeded, and the circulation uninfluenced; in the other the expansion of the chest is absolutely impossible, and the circulation is forcibly arrested: in one case—that of asphyxia—death is ascribed to the forcible stoppage of the circulation; in the other it is due to the altered character of the blood, which is made to circulate through the nervous centres.

It may also be observed, that as the phenomena and modes of death are entirely different in the two classes of cases, so also the order in which the cessation of vital functions is manifested to us is actually reversed: for in an animal which is "narcotised" to death, the act of breathing ceases first, and the heart's action is found to continue for a short period after the respiratory movements are at rest; but in an animal which has been suffocated, a few feeble inspirations may be observed after the cessation of the heart's action.

So also are the hopes which we may entertain of recovery very different in the two cases; for this may readily take place from a state of even the deepest narcotism,—for the heart is still in action; but scarcely can it be expected from that state which, in the strict meaning of the word, is "asphyxia,"—when the heart has already ceased to beat.

Now it appears to us that the vapour of chloroform, in some of its physiological effects, is exactly analogous to carbonic acid and many other irrespirable gases: like them, it is respirable, and capable of inducing death with all the phenomena of narcotism when admitted into the lungs under certain conditions of dilution; like them, also, it is irrespirable when air is saturated or very highly charged with its vapour, producing symptoms which cannot be dis-

tinguished from those of suffocation or strangulation,—the same congestion of the head and face, the prominent or staring eyes, the turgid veins, the struggling or convulsive spasms, the same ineffectual effort to expand the chest, the same rapidly fatal termination.

That the vapour of chloroform, in certain states of concentration, is absolutely irrespirable before anæsthesia is induced, is a truth of which we may at any time be satisfied by making the experiment on ourselves. We find, indeed, on attempting to inhale it, that we are immediately restrained in the effort; that the pungency of the vapour produces that feeling which is described as spasm of the glottis; and that the expansion of the chest involving the inhalation of the chloroform is, however much we may desire it, absolutely beyond our power. Anyone who will take the trouble to run through the observations on chloroform contained in the periodicals of the last six years, will find a large amount of evidence clearly pointing to this conclusion, and capable of producing a conviction which should be the stronger from the incidental manner, as it were, in which the evidence would come before him; for the facts which justify our conclusion, being too striking to escape observation, have been recorded, not indeed with the intention of proving the point for which we contend, but generally having reference to something else, or merely to give a circumstantial account of what took place on any occasion of danger.

It may be worth while, as the views now submitted differ considerably from those which have been urged by others, that I should give one or two illustrations of the evidence I allude to, which is sprinkled over a large mass of papers.

In an able essay by Dr. Snow, the following passage occurs:—"There is generally no alteration in the complexion of the patient, or in the colour of the mixed venous

and arterial blood as it flows from a wound, so long as the inhalation is not pushed to the extent of *embarrassing the respiration, and provided the patient is not holding his breath on account of the pungency of the vapour.*" Does not this passage clearly point to a state of concentration of the vapour of chloroform, which, from its pungency, renders the air which carries it to the lungs irrespirable before anæsthesia is induced, so that the patient is compelled to hold his breath? Surely, if this condition of things were continued by the lint or napkin, or other mechanical apparatus, being held over the mouth and nostrils, whilst all resistance on the part of the patient was rendered ineffectual by the assistants, would not the patient be suffocated or asphyxiated in exactly the same manner, and with precisely the same symptoms, as he would be if his head were immersed in water, or a pillow tied over his face?

Another writer—Dr. Young, of Peveril—gives his testimony to the same effect. "I find," he says, "that the first effect of the chloroform is to produce a painful sense of suffocation, with violent instinctive efforts to resist the further inhalation of the remedy. The sensation is described as that of drowning, and the propensity to struggle against it is irresistible. It is true that these efforts cease, and the anæsthetic state is induced, when the subject is compelled to continue the inhalation; but it is not always easy to accomplish this, and I have sometimes been obliged, by the frantic struggles of the patient, to abandon the attempt."

This passage so graphically describes what takes place in the attempt to force a person to breathe an air which, from its pungency, is irrespirable before anæsthesia is induced, that to comment on it seems superfluous. It points to the painful sense of suffocation, to the violent instinctive efforts at resistance, the sensation of drowning, the irresistible propensity to struggle,—in a word, it presents us with all the

phenomena of suffocation, just as if it had been intended, though this was not the case, to give us a picture of asphyxia.

Let us now revert to the question originally propounded: "Are the symptoms before death those of syncope, or paralysis of the heart?" or, "Are they not rather those which indicate the struggle of impending suffocation?"

It would appear indeed from the observations we have quoted, that chloroform does not prove fatal by causing paralysis of the heart, consequent on its too rapid absorption; indeed, we might rather conclude that death has taken place, not from the patient having inhaled *too much* chloroform, but rather from his having inhaled *too little;* that is, from the vapour of chloroform having been presented in too concentrated a form, its admission to the lungs was rendered impossible on account of its pungency; the respiratory movements have been altogether arrested, and the narcotising vapour has not even reached the absorbing surface of the lungs.

Thus far I have endeavoured to show that death by chloroform, when such symptoms as those narrated have been observed, is not justly attributable to paralysis of the heart, understood in such a sense as to exclude the influence of arrested respiratory movements in producing this result.

It will further appear, that as the symptoms before death direct us to the explanation afforded by asphyxia, so does the condition of the organs, as shown by post-mortem examinations, carry us to the same conclusion. These examinations disclose the fluidity of the blood, such as it is found in death by asphyxia; the deep congestion of the lungs; the distension of the right chambers of the heart, and the emptiness of the left; and the general venous plethora of the contents of the cranium and of the abdominal

viscera. In a fatal case which occurred at Auxerre,* it is remarked that the countenance just before death became deeply livid; and we read in the notes of the post-mortem examination, that "the lungs posteriorly, and the bronchial lining throughout, were highly congested; the blood was also found in a fluid state."

Professor Simpson, in commenting upon a case which occurred in the North of England, was so impressed by the condition of the post-mortem appearances, that he ascribed the death to suffocation caused by a small quantity of brandy and water which was given to reanimate the patient, and which he supposed to have passed into the trachea. There was, however, nothing in the record of the case to justify such an inference; and Dr. Glover, in a reply to his comments, instituted a series of experiments in which he showed that in death which was rapidly caused by chloroform, and in death by drowning, the post-mortem appearances were identical.

But if the views which I have brought forward be correct, it will naturally be expected that they should derive their principal support from the recorded cases of fatal occurrences,—for these ought to afford at once both the proof and the illustration of the principles for which we contend. We should expect to find that the symptoms which have been observed have not been those of a slowly induced narcotism, becoming gradually deeper till its full power has been manifested in stertorous breathing; we shall have no account of the incoherent ramblings of delirium,—for as the respiration has been arrested, all power of utterance has been taken away; if the pulse has been watched, it will be found to have ceased all at once; struggling will be a prominent feature in the record, for the

* Med. Gaz. vol. xlii. p. 108.

patient will have evinced intense earnestness to escape from the attempt at breathing a vapour whose pungency made it irrespirable before anæsthesia was induced. We shall find the deep congestion of the head and face, the fixed and staring eyes, the turgid veins; the convulsive struggle; the outstretched limbs, and their sudden relaxation;—in a word, the picture to be presented to us will not be that of a slowly induced narcotism; still less will it be that of syncope or paralysis of the heart, without any premonition; but it will be that of asphyxia with all its distinctive characteristics.

Let us see, then, how the case stands. The first case which I shall quote in illustration of our views is one which occurred near Gateshead, and is recorded in vol. xli. of the *Medical Gazette*. The following statements are made: "After drawing her breath twice, she pulled my hand from her mouth; in about half a minute, seeing no change in her breathing, or alteration of pulse, I lifted her arm, which I found rigid. I looked at the pupil, pinched her cheek, and, finding her insensible, requested Mr. L. to begin the operation. At the termination of the incision she gave a kick or twitch, which caused me to think that the chloroform had not taken effect. I was proceeding to apply more to the handkerchief, when her lips became suddenly blanched, and she spluttered at the mouth, as if in epilepsy. The usual efforts were made to restore her: she was laid on the floor; a vein in the arm, and also the jugular vein, were opened, but no blood flowed. The whole process of inhalation, operation, venesection in two places, and death, could not, I should say, have occupied more than two minutes."

As this was one of the earliest fatal occurrences which happened in this country, Dr. Snow, who was then pursuing his inquiries, wrote to the gentleman who had charge of the case, requesting further particulars, and asked a question

about "a moan" which had been mentioned by a witness at the inquest. The answer which Dr. S. received was that the so-called "moan" was rather "a prolonged forced expiration or splutter,—breath, saliva, and tongue being forced out at once, as it were: the sound was similar to the expiration in epilepsy." Is there anything in this case which is at all correspondent to "paralysis of the heart?" On the contrary, does the description leave it possible for us to escape the conviction that death took place by asphyxia, caused by an attempt to administer chloroform in a state of concentration which rendered it irrespirable, on account of its pungency, before insensibility had been induced.*

Besides, if the symptoms which were attested do not justify the inference of "paralysis of the heart," neither does the condition of the lungs as discovered after death. Sir J. Fife, who gave evidence on the case, thought that the immediate cause of death was congestion of the lungs,—which shows that they were affected in the same manner as would be produced by drowning, or any other method of suffocation.

In another case it is mentioned that "the patient struggled so, that the limbs escaped from the hold of the assistants, who, however, seized them quickly, and replaced the patient in his position." These are the violent efforts which are made to escape from impending suffocation.

In a third case, half an ounce of chloroform failed to produce anæsthesia, having caused only the ordinary *excitement and struggling*. After a delay of two hours, more was procured, and half an ounce was again applied on a hand-

* Nothing can be more remote from the writer's intention than to impute blame to anyone who was concerned in this or in any other case which may be cited by way of illustration. The just reputation of all to whose cases allusion may be made, is a sufficient protection against any such charge.

kerchief, care being taken to allow the entrance of air at short intervals.* Insensibility was induced, and a toe was amputated; but at the close of the operation no blood escaped,—which circumstance may lead us to suppose that death had already taken place, and that the insensibility was not that of a transient narcotism.

We have here the excitement and struggling: May not this struggling have been one of those vehement efforts which are made by the patient, as described by Dr. Young,† and not that state of partial rigidity which sometimes precedes complete insensibility?—the failure of effect, though much chloroform was used; its not producing insensibility on the first occasion, and at the second causing the death of the patient. Is it not quite as likely that the vapour of chloroform was presented in a state of too great concentration to admit of its being inhaled on account of its pungency, even though not absolutely irrespirable, as that it should have been inhaled to such an extent as is mentioned, and at last suddenly prove fatal,—not, indeed, through the intervention of its proper narcotic influence, but by paralysing the heart?

A fatal case which occurred at St. Thomas's Hospital

* We may here remark, by way of precept, that it is not sufficient for the purposes of safety to admit air at intervals only, and then to give the chloroform in its full strength. The end to be obtained—*i. e.* anæsthesia—is only to be gained with safety by giving the chloroform continuously, but sufficiently diluted. Dr. Snow has laid down the same rule of practice, but he rests it on a different *scientific* basis. "It is not," he says, "sufficiently understood—or, at least, borne in mind—that the vapour of chloroform requires to be largely diluted with air, not for the purposes of respiration (its physical constitution ensures that), but to prevent its operating with dangerous rapidity." Our difference on this point, though our precepts are identical, involves the whole question between us.

† Quoted in a former page.

presents the same feature of struggling and death within a minute.

In a case which happened at Berlin, five attempts were made to produce insensibility,—but in vain. *Almost at the commencement of the sixth, the patient stretched herself out, and was observed to froth at the mouth at the moment of death.* Though so little is reported of this case, more could scarcely be desired in confirmation of our argument, for the part which we have given in italics entirely falls in with the idea of death by asphyxia, rather than according to the other physiological view which would explain it by the supposition that chloroform paralyses the heart by its direct action.

In a case which occurred at St. Bartholomew's Hospital, it is mentioned that before the effect of chloroform was produced the patient had previously struggled much; and the resemblance which it offers to other cases more fully detailed would lead us to infer that the supposed effect of insensibility, when it was thought to have been produced, was in reality the insensibility of death; for no sooner had the surgeon made his incision through the skin, than it was stated that the pulse had ceased.

In the *Medical Times* of Dec. 1854, the following case is recorded:—Patient's age, 56. In the first instance about one drachm of chloroform was poured on the lint. The patient inhaled it kindly, and after about two minutes another drachm was added. A stage of excitement now followed, during which the limbs required to be held. Insensibility was just fully established, and the operator about to proceed, when the gentleman who was compressing the femoral artery exclaimed that the pulse had suddenly ceased. Examination at the wrist confirmed this remark. Almost immediately afterwards, a long-drawn inspiration, attended with a deep sighing noise, was observed. For two or three

breaths the cheeks puffed out during expiration ; the respiration next fluttered, then ceased. It is to be noted, that throughout the exhibition of the chloroform no change in the appearance of the patient was observed, *except that the superficial veins of the neck and temple became distended with blood.* The cessation of the pulse was most sudden, it was not preceded by the least premonitory fluttering,—the stroke of the pulse being good one beat, and entirely wanting the next. In the report of the post-mortem examination, it is said that the *lungs were much congested with fluid blood,* which ran out after incision of their substance. The left side of the heart was healthy, the *right distended with fluid blood.* As far as could be estimated, it was thought that the time spent in inhalation was about three minutes.

We will offer one or two observations on this case; and will first direct attention to the " stage of excitement" which was observed. Was this " excitement, during which the limbs required to be held,"—the rigidity which frequently precedes commencing narcotism; or was it not rather the voluntary effort of the patient called into action by his earnestness to escape from a vapour which he felt to be suffocating? We are scarcely left to conjecture on this point; for the casual mention of some most important symptoms compels us to adopt the last alternative: " the superficial veins of the neck and temple became distended with blood." Surely this is the venous congestion which results from arrested respiration,—an opinion which derives the fullest confirmation from the condition of the organs as disclosed by the subsequent examination.

We might quote other cases, each of which would furnish us with some one or more symptoms which point distinctly to arrested respiratory movements, or death by asphyxia, and are irreconcileable with the idea of syncope

or paralysis of the heart. We will, however, content ourselves with one of recent occurrence,* which appears to us to embrace all those distinctive features which have ever been held characteristic of death by suffocation. "Suddenly, it is related, symptoms of excitement occurred. The eyes became fixed and staring, the arms outstretched and rigid, and the face contorted. It was now impracticable to feel the pulse, on account of the tossing about of the arms; but, as is usual in such conditions, the respiration was noticed to be all but, if not quite, suspended by the spasmodic fixture of the chest. The patient's countenance changed somewhat during the treatment, but was mostly suffused and congested."

Now compare this recital with Shakspeare's description of the death of the Duke of Gloster, in which the poet contrasts the appearances which are well known to proceed from strangulation with those which would result from syncope or paralysis of the heart:—

"Oft have I seen a timely-parted ghost,
Of ashy semblance, meagre, pale and bloodless;
Being all descended to the labouring heart,
Who in the conflict that it holds with death
Attracts the same for aidance 'gainst the Enemy,
Which with the heart there cools, and ne'er returneth,
To blush and beautify the cheek again."

Then follows the contrast:—

"But see his face is black and full of blood,
His eye-balls farther out than when he lived,
Staring full ghastly like a strangled man:
His hair upreared; his nostrils stretched with struggling;
His hands abroad displayed as one that grasped,
And tugged for life, and was by strength subdued."†

* Med. Times, April, 14, 1855.

† 2nd Part of King Henry VI. Act 3, Scene 2.

It would be easy, but we forbear, to multiply cases in further illustration of these views. Let it suffice to say that we have not met with a single case in which some symptoms have not been recorded which point with more or less distinctness to arrested movements of respiration as due to the pungency of the concentrated vapour of chloroform. Thus, in one case it is said that the patient seemed "to be going into a fit;" and a neighbouring physician, who was immediately summoned to the case, said in his evidence at the inquest that "the face was livid."

In the case of a patient named Simmons* we read that "in two minutes the arms became rigid; the face, previously pale, became *livid*;" and again, "some slight removal of the *lividity* of the countenance was effected by the artificial respiration." It is well known, that convulsions, as of the epileptic, are attended by an arrest of the respiration; we are accordingly justified in inferring the latter when the evidence has been distinct as to the former. Thus, in the case which happened at Boulogne,† it is mentioned, "immediately on the respiration of the vapour the patient evinced agitation by moving the hands convulsively: this agitation quickly ceased, and she became motionless," &c.; and again, in a case at Hyderabad, it is stated, "She coughed a little, then gave a few convulsive movements, &c."‡

We consider, then, that the views which we have advanced on the subject of death by chloroform receive their highest sanction from the recorded cases of fatal occurrences, and that we are justified in our conclusion that chloroform has proved fatal, not, indeed, in the manner of paralysing, or narcotising the heart by its direct action, but by its influence in restraining the respiratory movements at the earliest

* Med. Gaz. vol. xlii. p. 80.

† May 4, 1848.

‡ Med. Gaz. vol. xlii. p. 84.

periods of its being administered, when its pungency would suddenly arrest its inhalation, and thus be the means of asphyxiating the patient with all those distinctive phenomena which we have already dwelt upon.

In adopting this view, we do not contend that the vapour of chloroform in any possible concentration is absolutely irrespirable under ordinary circumstances, but only that it is virtually so, and that, by reason of its pungency in the first moments of its being administered, and before a certain degree of insensibility has been induced. Hence we believe, and daily experience confirms us in our judgment, that a state of concentration of the vapour of chloroform, which at first would suffocate or asphyxiate a patient, may, after insensibility has been partially induced, be administered not only with safety but with advantage. Nor is this a mere supposition only, to be derived as an inference from the position we have taken; for a case has actually occurred which seems to furnish the very proof we might require, and is irreconcileable with any other consistent view of the subject.

As it frequently happens in the record of events that a circumstance will sometimes be mentioned in the most casual manner,—as if it could have no reference to the question under consideration, though in reality it may be the means of determining its most important issues—so, in the account which has been given of a case already quoted by us, we mark the statement of an occurrence of pregnant significance, on which we will offer a few observations. The circumstance we allude to is the use of the same inhaler in a case immediately before that in which it proved fatal. The points of the case may be thus given:—

Two patients in good general health had each to undergo an operation. An inhaler of excellent construction was used, and was first carefully regulated with a view to the

dilution of the vapour of chloroform. The first patient passed satisfactorily under its influence, and underwent an operation. The same inhaler was now used in the second case: but what effect was produced? Did the patient now pass slowly and gradually into the state of narcotism, as had happened in the preceding case, and as might have been expected, seeing that their circumstances were identical; for the mode of administering the chloroform did not appear to differ in the two cases? Quite the contrary. "Suddenly," we are told, "symptoms of excitement occurred: the eyes became fixed and staring, the arms outstretched and rigid, the face congested, and the respiration suspended by the spasmodic fixture of the chest." How could such an effect happen in the second case, and not in the first one? Was there any difference in the susceptibility of the two? We are not driven to—indeed we are forbidden to—entertain such an hypothesis, for, as the respiration was suspended by the spasmodic fixture of the chest, the inhalation of the chloroform became impossible by the arrest of that function which introduces it into the system; its pungency was felt at the glottis, and no inspiration took place.

The difference in the cases admits of the following explanation. In the first case the inhaler was so regulated that the chloroform passed to the lungs in a very diluted state; gradually, however, as the patient could bear to breathe it in a more concentrated state, the valve for the admission of atmospheric air was gradually closed, and the patient would thus be brought with greater rapidity, but at the same time with perfect safety, under the full influence of the narcotic. But if the same inhaler was now used in a fresh case, without the valve being sufficiently re-opened, or even if it was thus used again to the same patient, who, in the case of a prolonged operation, might partly recover

his consciousness, there would be the greatest danger of asphyxia in either case : and the experience of the profession has already furnished us with a fatal illustration in both these circumstances.

As the question we have been considering is of the highest importance in a medico-legal point of view, it may be interesting to examine the conclusions which were arrived at by the Commission which was appointed in Paris to enquire into and report on the fatal case which happened at Boulogne.*

In their first report they say :—

" In the medico-legal facts submitted to our notice, we have found no indication of the poisonous action of chloroform, and consequently we reply that the patient did not die from the effect of inhalation of that agent."

We agree with this conclusion ; for the patient showed no signs of the narcotic influence of the chloroform,—the symptoms were those of asphyxia, occurring at the commencement of the attempted inhalation : we say "attempted," for any real inhalation was rendered impossible by the suspended respiration.

The same Commission, in a second report, arrived at these two most important conclusions :—

1st. " Chloroform is liable to irritate by its odour and contact the air-passages," and

2ndly. " Certain modes of administration increase the danger : thus, there is risk of asphyxia when the vapour is not sufficiently mixed with air, or when respiration is not free." On which they founded the caution, " to take care that the respiration be entirely free."

Now, Dr. Snow controverts these conclusions of the French Commission. He cannot agree, he says, with the

* London Journ. of Med. p. 307.

first conclusion, which acquits chloroform of the death of M. Gorré's patient; and he refers what he considers to be an error in their reasoning, to the influence of a previous conclusion, viz. "that chloroform always produces intoxication and insensibility before death." The remarks which we have made in an earlier part of this paper, on the distinction between "death by asphyxia" and "narcotism terminating in death," will enable us to fix upon the points of essential importance in this argument.

The train of reasoning which was adopted by the Commission of Enquiry was such as the following: 'The effect of chloroform is to produce gradually a condition of insensibility, becoming deeper and deeper the longer the inhalation is continued. If death takes place without the intervention of the narcotic properties of the chloroform, or of any of those intermediate phenomena which would clearly be attributable to its influence on the system,—if, *à fortiori*, the symptoms which are observed are wholly opposed to the effects which are due to this agent, then in such a case we are not permitted to conclude that death has arisen from such a cause; we are thrown back in our enquiry, and the object of our search has not yet been laid open to us.'

Can we reasonably object to this mode of reasoning? Should we not be ready to admit its justice, if, in a supposed case of poisoning by prussic acid, or arsenic, or opium, it could be successfully shown that the symptoms which were noticed in the several cases were incompatible with those which naturally follow the administration of those poisons, and at once admit its claims to the acquittal of a defendant? But Dr. Snow contends that this mode of reasoning does not apply; and though he admits that the effect of chloroform when slowly introduced into the system is to produce the phenomena of narcotism, yet he argues, that, when very

rapidly introduced, it may suddenly narcotise the heart and that a patient may thus die through the sudden arrest of the heart's function by the direct action of this agent.

We have already given a partial answer to this argument by showing that narcotism or paralysis of the heart would be death by syncope, and that the symptoms in all the recorded cases forbid our accepting such an explanation.

But the question is deserving of a more special consideration; for an attempt has been made to give plausibility to the hypothesis of a 'narcotised heart,' by an endeavour to show that the circulation through the heart must be effected in a much shorter time than that through the brain, or any other more distant part of the body, and consequently that the priority of the heart's subjection to the influence of the chloroform is merely the natural result of the position it holds with reference to the rest of the system. It has thus been said: "The poison penetrates to the heart from the lungs in a single pulsation, and at the beginning of the next systole the blood is sent through the coronary arteries to the whole muscular tissue of the heart. The blood passing into the coronary arteries is less diluted, is more strongly impregnated with chloroform, than is the blood in any other part of the system except the lungs."*

Such a view of the physiology of the circulation has been advanced to explain how it may sometimes happen that syncope, or paralysis of the heart, as a supposed effect of the inhalation of chloroform, should overleap as it were the proper narcotic effects of that agent: how, as an exceptional occurrence, the heart should be affected before the brain, which, in ordinary circumstances, is the first to yield to narcotic influences.

Now this explanation, considered simply as a fact in physiology, must be either true or not. If it be true, it

* Med. Gaz. vol. xlii. p. 109.

must be obvious that it proves much more than is required; for it must apply not only to those cases in which the air is very highly charged or saturated with chloroform, but also to those in which the vapour is administered in a more diluted form,—if the explanation is applicable in one case, it must be so in every case; in the one which is normal as well as that which is exceptional. If the heart's relation to the circulation be such that it must, as it is argued, receive a supply of etherised blood in advance of the brain and other organs of the body, it must surely do so in every case, for the anatomical conditions of the organs cannot be altered; and yet, in the very deepest narcotism—even that which is carried on to a fatal termination—the heart seems to feel no influence beyond what might be considered due to the respiration, and will even continue to beat for a minute or two after this has wholly ceased.

But we deny the assumed fact in the circulation, which, though it has been maintained by some very eminent physiologists,* appears to us, when followed out to its legitimate results, to involve some contradictions which render the proposition untenable. As this point engaged our attention in a lecture lately delivered by us at the College of Physicians, we would beg to refer the reader to the argument used on that occasion, which is given in an Appendix.† We have therein contended, that if an artery bears any proportion to the organ it supplies—and no other supposition is allowable to us—it will result from such an admission,—the pressure on the blood, in the one case, and the capillary resistance, on the other, being equal,—that the circuit of the blood will be made in equal times through every organ of the body, whether this circuit be long or short.

* Keill, Müller, and others.

† See Appendix.

The doctrine of Keill and Müller assumes an equal rate of motion in all the arterial channels at an equal distance from the heart, which, if admitted, will imply an exact proportion between the sectional areas of arteries and their capillaries at *unequal* distances from these last vessels. But they are also forced to admit, if they allow that an artery bears any relative proportion to the organ through which it is distributed, that there must be a like proportion between the transverse section of arteries and their capillaries at *equal* distances from these last vessels. But these two ratios may be shown to be different; for no one denies the increase of the areas of arteries at each subdivision. Hence they are obliged to affirm this conclusion,—that two different ratios are proportionate to each other.

May we not say, then, that their proposition is untenable, —that, in fact, the blood reaches the capillaries of the brain, notwithstanding the difference in their relative distance, in exactly the same time in which it penetrates the corresponding system of vessels of the heart itself, and that it is no paradox in physiology, if we affirm that the heart, if we measure by time instead of by distance, is as near to the capillaries of the feet as it is to those of its own walls?

But to return to the point on which we were attempting to arbitrate between the conclusions of the French Commission and Dr. Snow's criticisms. How may we reconcile the views of both or of either of these with the truth? The Commission, on the one hand, denied that the patient's death was due to the action of chloroform, and added that there is in all cases risk of asphyxia when the anæsthetic vapour is not sufficiently mixed with air." Dr. S., on the other hand, contends that the danger is not one of asphyxia, but of over-narcotism,—"a narcotism which, according to circumstances, may cause death by paralysing the respiratory movements, and so bear a certain resemblance to asphyxia;

or may arrest the action of the heart, and so resemble syncope."

The argument of Dr. S. is this:—"There can be no danger of asphyxia, because the physical properties of chloroform render such a result impossible, since in any possible concentration of the vapour of chloroform only a small percentage of air is excluded by the presence of the anæsthetic vapour; far less than what may be shewn to be sufficient to support the process of respiration. But may we not reply to this argument—We agree with you that the vapour of chloroform does not act in its higher conditions of concentration by exclusion of air, and so give rise to the phenomena of asphyxia; but may it not act with equally fatal effect, if, though not irrespirable *per se*, it is rendered virtually so by its pungency, when, in the earlier moments of its being administered, it may, if not carefully regulated, restrain the respiratory movements, produce congestion or lividity of the face, and all those other symptoms of such deep significance? Were this otherwise, what would be the value of that excellent caution, "that chloroform should be administered gradually, much diluted with air at first, and less so afterwards?" Were this otherwise, what heed need we give to your own excellent observation, "that the patient sometimes holds his breath on account of the pungency of the vapour?"

We hold, then, that the French Commission pointed out the true source of danger, in referring the fatal issue of the case which was submitted to them to the class of deaths by asphyxia, and that the answer of Dr. Snow, though true as a fact in Physics, was irrelevant to their conclusion, which pointed to death by asphyxia, not indeed because the anæsthetic vapour as administered was absolutely irrespirable, but because, when insufficiently mixed with air, it is liable to irritate the air-passages by its odour and contact, and thus interfere with the freedom of the respiration.

We are now in the position to answer the important question which we have propounded,—"How may we ensure safety in the administration of chloroform?" Surely the answer to a question of such moment must not rest on the narrow basis of any one man's practical experience; it must be consonant with the experience of all; in accordance with the results of analogous cases, and lie on the foundation of sound principles of physiology.

The answer to the question must depend upon the answer which should be given to the preliminary one, "What is the real source of danger?" We have accordingly dwelt at some length on this part of our enquiry, and have endeavoured to shew with some variety of proof and illustration in what this danger consists; and that it is the danger of asphyxia, not that of narcotism. In our treatment of the subject it was incumbent on us to show that a certain concentration of the vapour of chloroform, though not irrespirable in itself and in all circumstances, had yet the power of asphyxiating; and the proofs which we offered of this statement were educed from the testimony of others in almost as high a degree as from the evidence of facts themselves. It is in this very point that the secret of all danger is to be found. Chloroform is endued with no poisonous or noxious influence which admits of rapid development, and may thus paralyse the heart's action: it can only narcotise, and the phenomena it will give rise to will be only those of a gradually increasing insensibility, affecting the heart as the last function of all, and this only through its dependence on the movements of respiration. Chloroform, when absorbed by the blood and admitted into the system, can only narcotise; however rapid may be its absorption, its effects will still be the same: it may, indeed, produce its phenomena of insensibility with greater rapidity in one case than in another, but it cannot overleap these, and directly paralyse the heart. How, then, has it so often proved suddenly fatal? It is not

because it has been inhaled and taken into the system, but because its pungency has been the means of preventing this inhalation, and at the same time of arresting the respiration. Its very safety is in its inhalation,—its only danger in its non-inhalation. Any concentration of the vapour of chloroform which can be breathed is safe ; any condition of dilution which forces the patient to cough or to hold his breath is dangerous, and if persevered in for even half a minute may be fatal. When deeply narcotised, the patient can inhale the chloroform in its highest concentration, but, if this be forced upon him at the commencement, he will immediately experience the feeling of choking or suffocation. If his hands be held by the assistants, he will attempt to disengage them, that he may remove the apparatus from his face. If they should unhappily resist his efforts, his struggles for life will become intense; his face and neck will become deeply congested; his eyes fixed and staring ; the whole body will be rigid with spasm. But this will last but for an instant ; his strength is now subdued, and he is powerless. Shall the operation be now commenced, for he gives no proof of sensibility? His insensibility is that of death. His pulse has ceased for ever, and a gasp or two, if even these should be noticed, will be the last tokens of vitality. Can we say that in such a case as this,—and this is no exaggerated or unreal picture,—that the patient has been killed by chloroform absorbed into the blood, carried directly in its highest concentration to the heart, and poisoning it at the very source of life? Why, the chloroform has not been even inhaled : its pungency was felt at the glottis, and its inspiration was immediately arrested. The patient would have removed the apparatus ; but in this he was restrained. The struggle forthwith commenced, and up to the moment of his death not a single inspiration took place.

It must now be fully apparent by what conditions we

may insure the safe and easy administration of chloroform. We have only to attend to the breathing; we may disregard all considerations affecting the relative proportion of chloroform in the air which is breathed: to ascertain this in practice is a matter of almost impossible attainment, and can only be conveniently regulated in experiments upon small animals. But our attention must be wholly given to the breathing, from the observation of which we must not allow ourselves to be diverted for a moment. If the patient breathes easily he is in safety, whatever may be the amount of chloroform which is passing to the lungs. But if his breathing be attended by frequent coughing, and still more if it appear to be restrained with deepening turgescence of the head and face, we have before us the distinct warnings of danger, and unless we give immediate heed to them, they will be speedily realised.

Let us discard, then, the groundless terror that chloroform may prove suddenly fatal without any previous warning. These warnings have never been absent,—they have been even most distinct, but unhappily they have been misinterpreted, and consequently disregarded. Let us, therefore, whilst we would use every caution in the manner which has been indicated, be full of a well-grounded confidence for the future; for we shall be able to appreciate the signs of danger at the very instant of their occurrence, and thus be able to avert them before they are consummated in death.

APPENDIX.

THE equal pressure of the blood at every point of the arterial system, occurring as it does quite independently of the blood's motion, though so simple in the manner in which it is effected—being, as we have shown, the immediate effect of the law of hydrostatics, which demands an equality of pressure on the same horizontal plane—involves several consequences of sufficient interest to justify us in bringing them before your notice.

These consequences have not only been overlooked by some eminent physiologists, but contrary ones have been assumed, and dealt with as facts.

Neither may we look upon them, as at first sight we might be disposed to do, as involving only some speculative interest, which could have no practical value. Perhaps there is no truth in physiology which is wholly devoid of practical value; but in the case before us we are inclined to believe that some most important misconceptions with reference to fatal occurrences by chloroform will find in part their appropriate refutation in correct views on this very point.

It is said—and we dispute neither the fact, nor the inference which has been drawn from it—that the area of the arterial system goes on continually increasing with each subdivision, and, as a result of this, that there is a constant diminution in the velocity of the blood's movement the further it is removed from the moving power of the circulation.

To take an extreme case, Keill says: "The rate of motion in some capillary arteries is to that in the aorta as 1 : 5233;

therefore, while the blood in the most distant arterial ramifications traverses the space of one foot, it will in the aorta traverse 5233 feet But as the blood in the aorta is discharged at the rate of 73 feet per minute, it will traverse 5233 feet in an hour and seven minutes. Accordingly, in the very minute divisions, it will traverse one foot in an hour and seven minutes. Since, then, he continues, we must admit between the blood's greatest and least velocity as many degrees as there are divisions of the arteries, so we must not ascribe the least degree of velocity to all the evanescent arteries, but only to those which have at least 40 divisions between them and the great trunk. But the velocity of the blood is always proportional to the number of divisions of the artery. Therefore, in those small arteries which have a very early origin from the aorta, and after a few divisions discharge their contents into the veins, the velocity of the blood is not much diminished."

Other physiologists have expressed exactly the same view. Thus Müller says : " The time in which the blood performs its course from one side of the heart to the other varies much according to the organ it has to traverse ;" in illustration of which, he says that the blood which circulates through the vessels of the heart itself to its right side requires a far shorter time for the completion of its course than the blood which flows to the feet and back again to the right side of the heart. He then compares the circulation through the various channels between the two ventricles to a number of arches, varying in size ad infinitum, the smallest of which arches would be formed by the vessels of the heart itself.

Now it appears to us that there must be some fallacy in this argument.

Let us see how the matter would stand if the views we have already submitted were received.

We have shown an equality of pressure in similar circumstances throughout the whole arterial system, and this pressure is in all cases independent of velocity; so that, when a man lies on his back, the blood is pushed with equal force throughout the whole system of capillaries, for in such a position there would be no addition to be made in any part to the force which results from the elastic reaction of the arteries; but if the same man changes to the standing posture, the relation of force is immediately altered throughout the body, for, in addition to the elastic reaction of the arteries, you must now add the pressure of a column of varying height for the several parts of the body.

According to this view, it will be apparent, that, although the resistance is always the same, the force will be subject to variations; and thus the force will be frequently greater the further is the distance to which it is removed,—as in the feet and legs of a man standing upright; and the result of this will be, in opposition te the reasoning of Keill, a more rapid transit of the blood at that part.

So, again, if we admit that there is some relation between an arterial trunk and the capillaries which correspond to or are supplied by that trunk (and this postulate is not unreasonable, for we should ask for no other explanation why the femoral artery should exceed the brachial in size, or why this should be greater than an intercostal branch), it would seem to result from such an admission that the distance from the centre of the circulation would not affect the rapidity of the discharge through the capillaries, and therefore, that if the trunks which supply these were everywhere proportionate, each severally to its own system, the blood would pass through the vessels of the extremities in the same time as it circulates through the heart itself.

Hence it appears possible, notwithstanding a continually

increasing area in the arterial system, with its just inference of a continual mean rebatement of velocity, that the blood may be forced with even greater rapidity through the capillaries at a more remote part than that with which it moves through them in the immediate vicinity of the heart itself.

Again: equal quantities of blood pass through the aorta and the capillary arteries in the same time; and so, likewise, equal quantities of blood pass through each separate arterial subdivision, and the capillaries which are dependent on that subdivision. Accordingly, we get this proportion:

The blood passing through the aorta.	:	The blood passing through all the capillaries.	::	The blood passing through any branch.	:	The discharge through the capillaries of that branch.

This proposition is self-evident, and does not imply any exact geometrical ratio between the sectional areas of the several vessels; nor does it make any assumption with reference to velocity, which must of course bear some relation to area.

Now as the resistance at the capillaries is every where equal in similar circumstances, each separate branch cannot receive a greater proportion of each fresh supply sent forth from the heart, than the passage of that which is already in its vessels will allow; but these vessels are always full, and resist the further entrance of any fresh blood, with equal force throughout the whole system. Therefore, on a rough calculation, in a man of 160 lb. weight, with a heart of 8 oz., the coronary arteries would only receive as their share of the two ounces of blood ejected from the heart at each pulsation, about three minims. The next arterial trunk, the innominata, would in like manner receive only its proportion, and so on throughout the whole arterial system. The two ounces would, in this manner, be finally spread as it were with equal distribution over an extended

surface, to the expansion of which they had been gradually adapted by means of a rapidly increasing area.

But this does not prove, nor afford any probability, that the blood makes the circuit of the vessels of the heart or of those in its immediate vicinity with greater rapidity than it does the longer circuit of parts which are more remote. Therefore, whilst we do not question the fact of the gradually increasing area of the arteries, nor one inference, viz., that the blood must suffer a continual mean rebatement of its velocity by that increase, we cannot admit the reasoning of Keill that the velocity of the blood is equalised in the arteries at each subdivision, and therefore that it is not much diminished in those small arteries which have a very early origin from the aorta; nor can we adopt the statement of Müller, that "the blood which circulates from the left ventricle through the proper vessels of the heart to its right side, requires a far shorter time for the completion of its course than that which flows to the feet and back again."

In every case, the vessels leading to the capillaries may be regarded as reservoirs for the supply of these vessels; so that, if these are proportionate to each other, the reservoirs to their discharging channels, they will be emptied in equal times, and a portion of blood, as it is discharged from the heart, will make the circuit of the extremities in the same time as that portion which passes through only the vessels of the heart itself. It is, therefore, no paradox in physiology, if we say that the heart, if we only measure by time instead of by distance, is as near to the capillaries of the feet as it is to those of its own walls.

But we have followed out our own conclusion without exposing the fallacy of our opponent's arguments.

It seems to lie in this,—that it gratuitously and falsely assumes an equal rate of motion in the arteries at an equal distance from the heart: it is therefore on this point that we

would join issue. We agree with him that there is a constant rebatement of the mean velocity of blood in the arteries from their gradually increasing area, but we deny as a necessary consequence that the rate of its motion is *equally* diminished in each subdivision.

To explain, therefore, the full import of this assumption, which, though at first sight appearing not unreasonable, yet in reality involves consequences which render the proposition untenable, we will endeavour to illustrate it by a distinct reference.

Let us take any portion of the arterial system, say the aorta just before it gives off the arteria innominata; its area at this point will be less than the united areas of the two divisions; and it will follow as a necessary result, that the mean velocity of the blood in the two channels will be less than in the original one; but it does not follow that the velocity in the two shall be equal, unless we admit that there is an exact geometrical proportion between the transverse sectional area of the innominata in relation to its capillaries, and the sectional area of the aorta in relation to its system of capillaries. If we affirm this proportion at this point, then we shall be forced to deny the gradually increasing areas of the arteries at each subdivision; if we deny it, which is equivalent to accepting the doctrine of the increasing areas of arteries, we must accept whatever may be involved in this denial. There is surely no inconsistency in supposing that there may be so great a rebatement of the blood's velocity in the innominata from that which it had in the aorta before the innominata was given off, that the velocity of the blood in this last vessel may be much less than that which it has in the aortic arch; and the same considerations may apply to any other portion of the arterial system.

This mode of viewing the case will of course imply that

there is not an exact geometrical proportion between the transverse sections of arteries and their capillary systems at equal distances from the heart. To assume such a proposition would manifestly be most incongruous: if, indeed, the distances between these points and their capillaries were equal, then we will concede this proportion, for in such a case the circumstances would be similar. We are ready to grant that the ratio of the sections of vessels to their capillaries is geometrically the same in all cases at equal distances from their capillaries, but at this point only are you entitled to ask for this proportion: here only would such a proportion be consonant with the fact of the gradually increasing areas of arteries.

Therefore, if the coronary arteries, which are the nearest to the heart, contain only their just proportion of blood, may we not say that the remaining portion of the system contains also its just proportion for the whole remaining capillaries of the body? In other words, if the cubic contents of the coronary arteries are to the capillaries of the heart as the cubic contents of the whole remaining portion of the arteries are to the capillaries of the whole body, then it will follow as a necessary consequence, the force or pressure being the same throughout, that the circuit of each will be made in exactly the same time.

But let us deny this correspondence between the cubic capacity of vessels and their capillaries. We admit, be it remembered, a geometrical ratio between the size of an artery, *i. e.* its sectional area, and the capillaries it supplies, but we restrict this admission by the just limitation to similar conditions. Let us, then, suppose that the cubic capacity of the coronary vessels bears a greater proportion to their capillaries than the remaining portion of the arterial system does to the capillaries of the rest of the body. In such a case, it will inevitably follow that the blood will make the

circuit of the whole body in a shorter time than it will pass through the vessels of the heart to the right side.

If, on the other hand, the cubic capacity of the coronary vessels bears a less proportion to its capillaries than the remaining arteries to their capillary system, you must adopt, unless you can show some reason to the contrary, the same line of argument for the remaining trunks of the arteries; the innominata and its branches will be of less cubic measurement, in proportion to their capillaries, than the aorta from which they spring is to the remaining portion of the system; and so on, until at last you would get some remaining portion of the vessel altogether disproportionate to the organ it had to supply. We surely need not pursue any farther the effect of such an anomaly.

Hence we make no undue assumption in maintaining, in opposition to Keill, to Müller, and others, that the blood makes the circuit of the whole body in the same time as it makes the circuit of the heart itself.

Our conclusion is the inevitable consequence which may be drawn from equal resistance at the capillaries, and a just proportion between the ratio of the sectional areas of different arteries to the capillaries they supply, — not, be it observed, at equal distances from the heart, which is clearly inadmissible, but at equal distances from their capillaries.

But the conclusion of our opponents assumes, first, an equal rate of motion in all the vessels at an equal distance from the heart, which, if admitted, will imply an exact geometrical ratio between the sectional areas of arteries and their capillaries at unequal distances from these vessels; and, in the second place, he admits an equality in their ratios at equal distances from their capillaries, if he allows that an artery bears any proportion to the organ through which it is distributed. Hence he is obliged to affirm this conclusion, that two different ratios are proportionate to

each other; and he also admits and denies at the same time the increasing areas of arteries at each subdivision. Therefore the whole argument is untenable.

It will not, I trust, be thought that this argument involves no practical considerations. Our speculations, indeed, must never be governed by such principles; and it may, perhaps, appear that some erroneous views on fatal occurrences by chloroform have received some sanction from the doctrines which we have been endeavouring to refute.

It had been my intention, an intention which I must now abandon, to offer at the conclusion of this lecture some remarks on the subject of death by chloroform. I will now, however, briefly allude to the partial refutation of a false theory, which is afforded by the doctrine we have been endeavouring to lay before you.

It has been remarked in all fatal occurrences by chloroform that death has ensued in a very short time; that only a very small quantity of chloroform has been used;—and the explanation which has been commonly given of these cases is, that the heart has been suddenly paralysed by an overdose of the narcotic circulating in its substance, which effect, it is said, would naturally take place from the position of the heart with reference to the rest of the circulation; for—as it is argued—the time which the blood takes to perform its circuits must vary with the length of those circuits, and the circuit which it makes in passing through the heart being very much less than that in any other organ in the body, a much larger quantity of highly narcotised blood will circulate in and oppress the central organ, before the influence of the narcotic has had time to reach the other organs of the body.

It has therefore been concluded, on such a view, that death has ensued from a rapidly narcotised heart.

But if there is any truth in the doctrine we have urged

in opposition to that which would alone justify such an argument, it is obvious that such an explanation must fall with the false physiology which sanctions it, and that we are still left to seek for some other account of those phenomena which demand from us such anxious consideration.*

* Extract from Croonian Lectures, 1855.

LONDON:
WILSON and OGILVY,
Skinner Street.