LECTURE

ON THE

CHARACTERS OF THE ARTERIAL PULSE,

IN THEIR RELATION TO THE MODE AND DURATION
OF THE CONTRACTION OF THE HEART IN
HEALTH AND DISEASE.

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INTRODUCTION.

THE order of succession of the muscular movements which take place in the heart each time that it contracts, has been very variously represented by different physiologists. More than a century ago, it was the subject of one of the most celebrated controversies* recorded in the history of medicine. Now that the habit of medical debating (happily for science) is extinct, and that we profess to be guided rather by observation than by rhetoric, opinions are scarcely less divided. If one notion can be said to be more prevalent than another on the subject, it is that of Haller, who believed that there is a *complete alternation* between the contractions of the auricles and ventricles, and that the time occupied by each cavity in contraction is equal to that during which it dilates or expands. (Haller, *Elementa Physiologia*, 1757, vol. i, p. 399.)

A century before, a truer doctrine had been taught by our own Harvey, who showed that the contraction of the heart is but *one movement* in which both auricles and ventricles take part, and that during the interval between each contraction and its successor the heart is absolutely at rest. (Harvey, *Opera omnia*, Lond., 1776, page 33.) This observation of Harvey, whether outshone by the glory of his greater achievement, or merely smothered in the futile disputations of his successors, was certainly forgotten until the discovery of auscultation again drew attention to the subject. By none was its truth more early or more clearly recognised than by our distinguished Fellow, Dr. C. J. B. Williams, who, in his *Inquiry Respecting the Motion and Sounds of the Heart*, published in 1833, shewed that no other view of the mode of the contraction of the heart is consistent with the facts revealed by auscultation. Its most complete vindication is to be found in the classical essay of Dr. John Reid, in the *Cyclopædia of Anatomy and Physiology*, who gives the following view of the succession of events in a cardiac revolution: "When the heart of a living animal is exposed, and the organ is acting in a natural manner, the auricles are observed to become distended with blood, then to contract rapidly and simultaneously, and propel part of it into the ventricles; this is accompanied with a corresponding enlargement of the ventricles, which is immediately followed by their simultaneous contraction and the propulsion of their blood along the large arteries; then follows a pause, during which the auricles become gradually distended by the blood flowing along the veins. When the auricles are filled, they again contract, and the same train of phenomena just described occurs in uniform succession." (*Cyclopædia of Anatomy and Physiology*, vol. ii, p. 602.)

Although the researches of later observers, and particularly those of Chauveau, are in perfect harmony with this account, there is a constant tendency to return to the doctrines of Haller and Laennec, which commend themselves to the mind by their extreme simplicity, and consequently to represent the duration of the contraction of the heart as something like half that of the whole period occupied in what is called a cardiac revolution—as being nearly equal, in fact, to the interval between one contraction and its successor.

There are several facts, not difficult of observation, which render this view untenable, and shew that the time occupied by the heart in contraction is very much shorter than is commonly supposed. The most obvious, and certainly not the least satisfactory, proof that this is so, is attainable to any one who is capable of measuring short intervals of time by the ear with approximative accuracy, by listening to the sounds of the healthy heart. The first sound of the heart being synchronical with the commencement of the contraction of the ventricle and the closure of the mitral valve, and the second with the closure of the aortic valve, it is

obvious that the interval between these two events expresses the duration of the contraction of the heart. Now, the most unpractised auscultator can readily satisfy himself, while listening to the sounds of a heart contracting sixty times in a minute, that the time between the first sound and the second is not equal to that which separates the second from the first, and that it cannot for a moment be admitted (as stated in some of our leading physiological text-books) that the heart occupies half a second in contracting. (See Dr. Kirkes's *Physiology*, p. 87.)

But the most positive and conclusive proofs of the brevity of the systole of the ventricle are obtained by direct observation of the movements of the living heart in animals. Harvey founded his doctrine on the subject on what he saw and felt when the living heart was exposed and seized in the hand. He observed that the heart, in beginning to contract, becomes round and hardens, and that these changes of form and consistence occur with such rapidity as to appear instantaneous. In the recent experiments of Chauveau, already referred to, this evidence is rendered more precise, though not more positive, by the introduction of those appliances of measurement which distinguish modern physiological research. Chauveau measured, by a recording dynamometer, the tube of which terminated in an elastic bag introduced into the left ventricle of the horse, the exact time occupied in its contraction, and found it to be about a fifth of a second. The observation of the pulse by the sphygmograph is another method of the same kind; the function of that instrument being to record changes of the same nature as those registered by the dynamometer; for, in the sphygmographic observation, these movements of the heart are communicated to the recording lever by that artery, just as in Chauveau's experiment they are transmitted by the flexible tube.

It is the purpose of the present lecture to shew that the sphygmograph, regarded as a dynamometer, is not merely an aid in physiological research, especially with reference to the question of the duration of the successive acts which constitute a cardiac revolution, but that in it the pathologist is furnished with an instrument whereby he can investigate the mode and measure the duration of the ventricular systole in disease. With this view, I shall endeavour to explain, as clearly as I can, the meaning of the record inscribed by the lever, or, in other words, the relation between the *form* of the tracing and the *movements* of the heart of which it is a representation.

In every arterial pulsation, four events are to be distinguished.

1. *The Sudden Primary Expansion of the Artery.* At the moment when the ventricles contract, all the arteries suddenly expand. This expansion does not, however, express an increase in the quantity of blood contained in the artery; for it can be produced by any sharp movement, of however small extent, provided that it is of great rapidity and is directed in the axis of tube. It is due to a sudden jerking forwards of the particles of liquid, and is, therefore, of the nature of a commotion or shock in which each particle communicates its movement to its neighbour—just as when a number of balls are freely suspended in a straight line in contact with each other; a smart rap given to the terminal ball shews its effect, not in any advance of the whole number, but in the jerking forward of the ball at the opposite end of the series.* This primary effect of the heart on the arteries, although so sudden as to appear instantaneous, is not in reality instantaneously transmitted. By experiments, which will be elsewhere published, I have ascertained that, in elastic tubes containing water, at a pressure nearly equal to that commonly existing in the arterial system, sharp propulsive movements are propagated at a rate of about ninety feet per second, so that the time which intervenes between the shock of the heart, and the consequent sudden expansion of the radial artery, probably does not exceed a fiftieth of a second, a period altogether inappreciable without the aid of instruments.†

Thus, the first effect of the contraction of the heart on any artery consists in the rapid transmission of an oscillatory movement through the liquid contained in it in such a way that, although every part of the artery partakes sooner or later of the expansive movement produced, no two parts expand at the same moment. From the nature of the move-

* To this illustration it was rightly objected by a Fellow, who did me the honour to be present at the lecture, that in the case of the experiment with the balls, the effect in question is not observed unless they be made of elastic material—e.g., of ivory; if they be of clay, the movement of the terminal ball is of no greater extent than of any of the rest, there being no propagation of movement from ball to ball. Water being an inelastic fluid, its particles are analogous to the clay balls, not to the ivory ones. The answer is, that inelastic liquids, when enclosed in elastic receptacles, have the same properties, as regards the transmission of molecular movements, as if they were themselves elastic.

† The laws of transmission of this movement are those which regulate the transmission of all oscillatory movements in liquids enclosed in elastic receptacles, particularly in elastic tubes. They were first investigated by E. H. Weber, to whose essay (*De Pulsu, Resorptione et Tactu*, Leipzig: 1834) the reader is referred for further information; the most important of them is, that the velocity of propagation varies as the square root of the elasticity—i.e., inversely as the square root of the elastic yieldingness of the tube.

* I refer to the discussion between the rival schools of Paris and Montpellier.—See *Mém. de l'Acad.*, vol. i, 1743.

ment, it further results that it is *repeated*. Each primary expansion of arterial wall at any given point is immediately followed by an equally sudden contraction, and then by a second expansion and contraction of less extent, and these by a third, and so on; so that the artery is thrown into a state of rapid vibration, which is more or less marked, according to the suddenness of the shock of the heart and the elastic yieldingness of the arterial wall. (See Figs. 1 and 2.)

2. *The Second Event* is indicated in the sphygmographic tracing by a curved line, the concavity of which is downwards. Its normal form is shewn in the dotted lines in Figs. 1, 2, and 6. It is that part of the tracing which is most subject to variation. In many varieties of febrile pulse, it is entirely absent; in the pulse of arterial resistance, usually met with in hypertrophy of the left ventricle, it attains its greatest development. (Figs. 3 and 4.)

Its physiological significance is great, for it denotes the degree and duration of distension produced by each contraction of the heart, in the particular artery to which the sphygmograph is applied. Pathologically it is no less worthy of attentive consideration; for it expresses an important quality recognised by the finger in disease, that of *fulness*.^{*} Let us consider what this familiar term signifies. Fullness is supposed to mean, that at each stroke of the heart a large quantity of blood is discharged; or, in other words, that the contraction of the left ventricle is unusually effectual. But this is not quite true, or, if true, does not express the whole truth; for, in all those conditions of the circulation which, with strict accuracy, may be termed *atonic*, the volume of blood which passes through the aortic valve at each beat is large, yet the pulse has a character very different from that of fullness. Again, the fact that fullness of pulse is so frequently met with in old persons cannot be supposed to imply that the contractions of the heart are more effectual towards the close of life than at an earlier period.

The explanation is, that fullness of pulse does not indicate that the artery in which it is produced receives more blood absolutely, but that it receives more in proportion to its capability of disposing of it. The blood which enters the arterial system is disposed of in two ways. Some of it is immediately transmitted through the capillaries, while the remainder is at first retained within the arteries and added to the quantity previously occupying their cavities, to be subsequently (*i.e.*, during the diastole of the heart) forwarded to the veins. The proportion which the quantity of blood immediately transmitted bears to the quantity which remains in the arteries, depends on the vigour of the contraction of the heart and the state of the contractile capillaries, both of which conditions are subject to great variation. The more readily the capillaries allow blood to pass, the more rapidly do the arteries empty themselves, supposing that the activity of the heart is unaltered. If, on the other hand, the force of the ventricular systole be increased, the effect produced on the arteries is the same as if the capillaries were narrowed; for they at once become insufficient for the transmission of the quantity of blood thrown into the arterial system by the heart. In either case, there is *delay in the collapse of the artery*, which manifests itself sphygmographically in what I have called the *second event*. The artery is distended, not only at the moment that it first receives blood from the heart, but *continues distended* for a longer or a shorter period afterwards. This continuance of distension has always the same meaning. It denotes the resistance afforded by the capillary network to the circulation during the systole of the heart.

It is well known that the *full pulse* (sphygmographically, that in which the second event is well marked or developed) occurs in very opposite states of health, or rather of disease. From what has been stated above, it may be readily understood how this happens; for, if the explanations given are correct, protracted arterial distension may arise, either from excessive vigour of the heart's action, from *narrowing of the capillaries*, or from disease of the arteries themselves—conditions which are essentially different. Thus, you have at least three forms of full pulse: the full and hard pulse of old age; the full and soft pulse met with in certain forms of febrile reaction, and especially in diseases of the nervous centres; and the full and firm pulse of vigorous health.

The sphygmographic distinction between these three forms will be more completely given after we have considered what takes place during the period which corresponds to the repose of the heart, to the consideration of which we now pass.

3. *The Diastolic Collapse, or Third Event.* In studying the arterial movements which occur during the diastolic interval or pause, the commencement of which is followed by the closure of the aortic valve, the same distinction is to be drawn between those effects which are due to changes of blood-tension and those which are vibratile. Just as the con-

traction of the heart produces a sudden, merging into a more gradual expansion of the arterial system, so the *cessation of contraction* manifests itself in a shrinking or collapse, which is more or less violent, sudden, or gradual, according to the state of the circulation; or, adopting instrumental language, just as the bursting open of the aortic valve is indicated by a vertical ascending straight line, followed by a curve of which the concavity looks downwards, so the movement which leads to the closure of that valve is expressed by a vertical descending line, ending in a curve of which the concavity is upward.

The counter-shock or recoil produced in an elastic tube through which a stream of water is passing, by suddenly arresting the current, is equal in intensity to the shock which occurs when the stream is first set in motion, and the two movements are of the same kind, the only difference being that, in the latter case, the direction of the primary movement coincides with that of the stream, and shews itself in the sudden expansion of the tube, whereas, in the former, it is against the stream and produces contraction. If, therefore, the velocity of the blood stream in the aorta were the same at the end as at the beginning of the systole, the shock at the end of the systolic period would be of the same intensity as at its commencement. The conditions of the circulation are, however, such that this could not possibly occur. For, inasmuch as the left ventricle gradually empties itself of blood, the force with which it compresses its contents gradually diminishes during the progress of the contraction, while the resistance in the aorta increases. Hence, at the moment that the contraction ceases, the rate at which the blood flows through the sigmoid orifice has already undergone considerable diminution.

The collapse of the arterial system being the immediate result of the cessation of the contraction of the heart, the determination of the time at which it occurs is of the greatest importance; for it affords a means of measuring the duration of the contraction. As we shall see further on, the classification of pulses depends upon it, not merely in a sphygmographical sense, but with reference to ordinary characters, recognised by the finger. In investigating the pulse by the finger, the physician judges of its character by the time the artery remains expanded, just in the same way, though not so accurately, as if he measured its duration by the sphygmograph.

4. *The Diastolic Expansion, or Fourth Event.* The fact that every artery again expands after the collapse at the end of the systole, is one which has occasioned much speculation, especially since its constancy in all conditions of the circulation has been known. In the pulse of adynamic fever, it has been long recognised as communicating to the pulse a special character, that of *dicrotism*. Its cause has been much discussed. According to Naumann, it is produced by what he calls the recoil of blood against the aortic valve; and the explanation of it given by Marey is not altogether dissimilar, although differently expressed. That such a recoil exists, we have already seen; but that this is the explanation of the subsequent expansion of the artery, I have myself disproved by numerous experiments, to which this is not the proper place to refer more particularly.

What, then, is the cause of dicrotism? Without entering minutely into the physical aspects of the question, it may be permitted to observe that, from the nature of the phenomenon itself, it implies that the disturbance of pressure-equilibrium in the arterial system, which is necessarily produced by the injection of blood from the heart, is excessive. For, if it were otherwise, the arterial distension would subside gradually and uninterruptedly, and be expressed by an unbroken curve, the form of which would correspond to the velocity of the capillary circulation.

Interesting as it would be to follow out these considerations, it is for the present of more importance to study its practical significance as a sign of disease. And this I hope to do satisfactorily by referring to my own observations as to the circumstances under which the dicrotic pulse is observed.

The states of the circulation which produce true dicrotism are just the opposite of those which favour the development of the second event. The one implies fullness of pulse, the other the contrary. The one expresses the continuance of the arterial expansion, the other its abnormal abbreviation. Both alike depend on the proportion which the quantity of blood projected by the heart at each stroke bears to the capillary resistance. The smaller the quantity thrown into the aorta by the ventricle, the sooner is it got rid of by the capillaries, and the more dicrotic does the pulse become. If, on the other hand, the capillaries are dilated, the effect is the same; for, in either case, the collapse of the artery is accelerated, and the period of its expansion abbreviated.

[The remainder of the lecture, which will appear in a future number, comprises illustrations of the various forms of pulse met with in disease. In order that the reader may have the principles I have endeavoured to establish in the introduction more clearly before him, and that the

^{*} See the full exposition of the characters of the pulse in disease, by Dr. Guy, at p. 115 of the *Physician's Vade Mecum*. Dr. Guy regards the terms full and large as equivalent—both signifying that the quantity of blood expelled at each contraction of the heart is excessive.

who are beginning to use the sphygmograph may be furnished with a kind of grammar of its language, I submit the following series of propositions as explanatory of the text.

It cannot be too often repeated that, with the one exception of aneurisms, the sphygmograph is not an instrument of diagnosis; for affections the most diverse communicate to the pulse the same graphical characters. Its use is to enable the physician to investigate the state of the circulation and circulatory organs in diseases of which the general nature is already recognised, with reference to (1) the mode and duration of the contraction of the heart, (2) the soundness of the arteries, and (3) the relative quantity of blood contained in the arteries and veins, or, in other words, the balance of pressure between the venous and arterial systems. If it can be shown that the sphygmograph affords a reliable means of determining these most important conditions of disease, it is quite enough, without forcing it to applications of which it is from its very nature incapable.

(1) The heart possesses in itself the power of contracting rhythmically. This faculty may be exercised either automatically, *i. e.*, independently of the central nervous system; or partly automatically, partly under the influence of impressions reaching it through the spinal cord. The researches of Bezold have shown that the nerves through which these impressions are conveyed are those which, originating from the spinal nerves and passing through the thoracic and lumbar ganglia of the sympathetic, converge to the posterior cardiac plexus. The same physiologist has also shown that it is through these channels that the modifications of the mode of contraction of the heart which result from emotion and suffering are induced, and render it probable that the perverted actions of the heart met with in fever and other constitutional disorders are also spinal in their origin.

A large number of facts have led me to the physiological inference that, *whenever the heart contracts of itself, i. e., automatically, it contracts gradually and peristaltically, its constituent fibres being brought successively into action; and that, in so far as the movement is deprived of its automatic character by the influence of stimuli acting through the spinal cord, it becomes sudden and instantaneous.*

Sphygmographically, suddenness of contraction manifests itself in verticality and amplitude of the primary ascent of the tracing; while in those forms of pulse which correspond to a more gradual mode of contraction, the first event is indistinguishable. Hence we are led to associate absence or suppression of the first event with all those conditions of the circulation in which the heart may be supposed to act automatically; and to believe that, whenever the artery expands sharply under the finger at the moment of the shock of the heart—whenever, in short, the first event becomes a prominent feature of the tracing—we have evidence therein that the systole of the ventricle is no longer peristaltic, but reflex; and that, through the spinal cord, influences are at work (whether originating from emotions, sensations, or abnormal constituents in the blood) which are not altogether normal. In all such cases, a sharp sound, having the character of the second sound, is heard on auscultation in the neighbourhood of the præcordial impulse. To this I propose to give the name of shock-sound: it is a sign of considerable importance. (Compare Figs. 1 and 2.)

Fig. 1.—Pulse of nervous fever ("prostration with excitement"), 100 per minute, observed in a case of tubercular pneumonia: a well marked sensation of thrill was communicated to the finger. The artery expands violently, and immediately afterwards begins to collapse; but the collapse is interrupted by a series of vibrations, which shew themselves distinctly in the tracing. The dotted line, commencing from the middle of the diagram and extending to the end, denotes the normal form.

N.B. In each figure, the vertical stroke indicates the closure of the mitral valve—*i. e.*, the commencement of the primary expansion, while the vertical dotted line denotes the closure of the aortic valve and the end of the diastolic collapse. That part of the tracing which immediately precedes the diastolic collapse is the second event. That part which immediately follows it is the diastolic expansion, or fourth event. All of the tracings are double the natural size.

(2) *Absence of Elasticity* is inseparably associated with hypertrophy and dilatation of the arteries, and increased arterial resistance. In the natural state of the circulation, the increased tension produced in the arteries by the ventricular systole manifests itself partly during the systole itself, partly during the succeeding pause; the agent in thus distributing the effect being obviously the arterial elasticity. When the arte-

ries are not elastic, this distribution of tension does not occur; and consequently we find the arteries relatively *more* tense during systole, *less* tense during diastole. Further, the increased resistance prolongs the systolic period. Hence the absence of elasticity is indicated sphygmographically thus. The diastolic collapse is deferred until immediately

Fig. 2.—Atonic pulse of a healthy person of sedentary occupation, residing in town. The primary expansion is too violent, while the second event is defective. Under the influence of an anodyne dose of morphia, it would assume the form indicated by the dotted line on the left of the figure; a moderate dose of alcoholic stimulant would give it the form shown on the right.

before the closure of the aortic valve (see Figs. 3 and 4)—a fact which is indicated in the tracing by a nearly vertical line. The length of this vertical descent indicates the amount of the sudden diminution of arterial pressure, which coincides with the cessation of the ventricular systole.

(3) *Relative Fullness of the Arterial System.* Inasmuch as the quantity of blood contained in the circulatory system is more or less constant, it follows that there must be an inverse relation between the quantity contained in the arteries and veins at the same time, so that when the one increases, the other must proportionately diminish. Now, it is the function of the heart, if one may so express it, to preside over this relation, by increasing the vigour of its contractions whenever the arterial pressure becomes insufficient for carrying on the circulation, and relaxing its efforts when the arteries become too full. So important, indeed, is the maintenance of the balance of tension between arteries and veins, that it involves the whole question of the competency of the heart to perform its mechanical functions.

Undue impletion of the arterial system shows itself in *postponement of the arterial diastolic collapse*, which means, as regards the heart, that, in consequence of increased arterial resistance, the left ventricle continues full for an unnaturally long period—not collapsing, indeed, until its muscular contractility becomes exhausted. In this state of the circulation, the first sound of the heart is prolonged up to the moment when the second sound is audible. (Figs. 3 and 4.)

Figs. 3 and 4.—Pulses of hypertrophy of the left ventricle, with dilated arteries. The one differs from the other in this respect only, that the primary expansion is more ample in Fig. 3, and that in Fig. 4 the diastolic collapse is not so sudden, indicating that the arteries are more elastic.

Insufficient impletion of the arterial system (supposing the arteries to be in a healthy state) is necessarily associated with *diminished elasticity*, *i. e.*, diminished elastic resistance, or, to use the converse expression, increased elastic yieldingness.* It shows itself sphygmographically by

Fig. 5.—Dicrotic feeble pulse of mitral regurgitation. It is seen to differ from the normal dotted line, principally in its great frequency, and in the depth and amplitude of the diastolic notch. This pulse closely resembles the undulating pulse of typhus. In the one disease the contractile force is *weakened*, in the other it is *wasted*. The effect is the same: the systole is ineffectual.

Fig. 6.—Dicrotic pulse observed during an attack of pneumonia in a man of feeble constitution. By comparison with the normal, which is represented by a dotted line commencing from the left of the figure, it is seen to differ, (a) in the too great violence of the primary expansion; (b) in the abolition of the second event; (c) in the postponement of the diastolic expansion. In pulses which are still more dicrotic, the diastolic expansion occurs still later, as shewn by the dotted line on the right. In such pulses, the administration of alcoholic stimulant shortens the interval—*i. e.*, brings the pulse nearer to the normal.

* Much confusion is apt to arise in the use of the word elasticity. Most persons understand by elasticity, yieldingness, and would consider a bit of whale-bone as more elastic than a steel spring. In physics, the word is used in the opposite sense.

a well-defined character—the *absence of the second event*, or true diastolic, which means that the diastolic collapse occurs immediately after the primary expansion (the vertical ascent of the tracing ending at once in a nearly vertical ascent), and that the contraction of the heart, however violent it may be, produces little effect in increasing the arterial tension. (Figs. 5 and 6.)]

ON THE INDICATIONS FOR MANUAL INTERFERENCE AND OTHER AGENCIES IN THE TREATMENT OF ABORTIONS, WITH CASES IN ILLUSTRATION.*

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I HAD looked forward, in the composition of this paper, to the pleasure and advantage of having our late friend, Mr. Hardey, as one of my hearers. His sound practical discrimination gave a high value to his opinion on matters with which his vast and varied experience made him familiar. But, alas! we are all deprived of the benefit of his remarks by his sudden and lamented death.

I most cordially subscribe to the views of Drs. Rigby, Denman, and Ramsbotham that, as a general rule, it is wise not to attempt the removal of the placenta by manual efforts in the abortions occurring during the first four or five months of pregnancy. I only venture to dissent from the dictum of those great authorities, when they assert that such interference is never necessary. They very properly dwell on the injurious effects from injudicious meddling, which they have witnessed. I myself once saw life seriously endangered from unauthorised efforts to remove the placenta about the fourth month of gestation. I found the patient greatly exhausted, partly from hæmorrhage and partly from the shock of the operation, which, to my perceptions, had lacerated the mouth and cervix of the delicate uterus. But, though placed in much peril for several days, she eventually recovered. La Motte also, in his 142nd Observation, furnishes another illustration of the danger of officious interference in the third or fourth month of pregnancy. Miscarriage had occurred to a young lady by her leaping from a restive horse. "They shewed me the child," says he, "which was exceedingly small, with a small end of the cord. I placed her in a convenient posture, and found the cord, which I followed to the entrance of the uterus, which was so closed, that I was hardly able to introduce my finger, by whose means I loosened the placenta from the sides; after which I made use of the cord, which was more serviceable to me than I could have expected. I helped at the same time with my finger, acting with it on all sides. At last I brought it away whole. But the lochia stopped, and a fever came on, from which she died fourteen days after the miscarriage." Now, here no excessive hæmorrhage warranted the operation, nor was time allowed for those expulsive efforts of Nature, which might have been solicited by the tampon or other expedients. With many such examples on record of rash interference, no wonder the best writers have agreed to proscribe it.

"I have seen many instances of the injurious effects of such attempts," observes Dr. Ramsbotham; and he corroborates his advice to leave the removal of the placenta to Nature, by citing a case in which a miscarriage of twins about the fifth month was not followed by the expulsion of the placenta before the lapse of three weeks, and yet he had refused to interfere. In contrast with this, he mentions the case of a very lovely woman, whose life was placed in great peril from a flooding induced by the ill-judged but ineffectual efforts of an ill-instructed accoucheur to remove the placenta between the fourth and fifth months. He remarks also, that he has known cases terminate favourably, in which considerable hæmorrhage has taken place in the interval between the expulsion of the foetus and that of the placenta; and he adds the comforting assurance, that "he has not met with one, in which similar symptoms of danger have succeeded the retention of the placenta at this period of pregnancy, as are met with under like circumstances at the full period."

It cannot be concealed, however, that Nature is not always competent to the management of these cases, when unaided; but that in rare cases death ensues from a portion of the foetus, or its involucre, remaining in the uterus. Thus, a case is related by Dr. George Pearson Dawson, of Sunderland, in his *Nosological Practice of Physic*, in which he was requested by his friend Dr. Monteath, in Ireland, to visit a woman in whom a part of the foetus, after abortion, remained unexpelled. Extrac-

tion was impossible, inasmuch as the os uteri was closed and undilatable. Neither of them could dilate the mouth of the uterus, which scarcely admitted the point of a finger. Death was the result. It does not appear, from the narrative, that either Dr. Dawson or Dr. Monteath was familiar with the virtues of ergot of rye. Notwithstanding the general truth of the impropriety of manual interference in the early months, we cannot conceal from ourselves the possible fact, that death may ensue, directly or remotely, from protracted hæmorrhage, or from the febrile irritation and disorder set up occasionally in the system by a retained placenta. That these are but exceptions to a general rule, I admit; nevertheless, as exceptions, they have a real existence, and are to be vigorously dealt with. Nor can any finer tact or judgment be displayed in the practice of our profession, than in singling out exceptions to general rules of practice, and dealing with them on peculiar principles. Nature, though often fully equal to the emergency, yet in feeble constitutions sometimes entirely fails, or only just rescues the patient's imperilled life from the destructive consequences of excessive floodings. In fact, a case occurs to my recollection, in the fourth month of pregnancy, in which the hæmorrhage was so tremendous, as both to endanger life and ultimate injury to the general health. It was a fifth miscarriage, and the woman had besides a numerous living family. I was not aware at that time of the virtues of *secale cornutum*. I scarcely ever witnessed so profuse a flooding at so comparatively early a period. On my first visit I ascertained, that the foetus had been expelled, but not the placenta. On examination *per vaginam*, I could just feel the placenta slightly protruding down the *collum uteri*, and nearly so far forward as the os tincæ. I attempted to lay hold of it with two fingers—the woman being faintish and the os uteri relaxed—but in vain. The attempt induced syncope. The hæmorrhage returned with severity on the following day, and lasted two hours. On the third day, there were slighter returns of hæmorrhage accompanied with diarrhoea. On the forenoon of the fourth day, the flooding became more profuse than ever, and was followed by severe and repeated syncope. All attempts by the hand proving abortive, I kept the tampon introduced, and adopted the usual remedies. I lived at that time at Burlington. In the evening, my late brother, who chanced to be over from Beverley, saw the patient with me, and urged the propriety of attempting to remove the placenta. Indeed, no one could doubt the imperative necessity for this practice, if it could be pursued without too much violence. We both made several attempts, as well by the fingers as by a pair of common surgical forceps, but were foiled. On the following morning, the alarm of the patient's friends was so great, that another experienced accoucheur (Mr. Dawson) was called in. He too attempted in vain to remove the placenta by his fingers; but urged a repetition of the attempt by the forceps at our next visit. On that occasion, I first essayed the operation by the oesophagus forceps, but failed. Mr. Dawson made a similar attempt by the common surgical dressing-forceps, but was foiled. My brother, whose surgical fingers possessed immense command in an operation of this kind, from their prodigious length, came next, and, by means of the shank of a table-spoon carried into the uterus so as to make with his finger a pair of forceps, he broke down a fragment of the placenta and brought it away. He had also probably caused the remainder to descend somewhat; so that, following up the advantage thus gained by his forceps-like fingers, he at length succeeded, by cautious but resolute and persevering efforts, in bringing the entire remaining portion away. The system was so prostrate, that little fear of inflammation was felt on account of these strenuous efforts to remove the placenta. Indeed, the hæmorrhage was so frightful, and life itself was in such peril, that we were all strongly impressed with the soundness of the principles and practice inculcated in these cases by Burns. Here, too, the placenta was forced down into the *collum uteri*, and seemed disposed to remain there, notwithstanding occasional paroxysms of after-pain kept up by a dilated state of the os uteri, and despite the influence of the tampon. No hæmorrhage followed the last successful operation, and the patient eventually recovered. The ergot of rye was not tried in this case; and conjecture as to its possible influence cannot neutralise the conclusion that, in such hæmorrhagic cases as this, manual interference is necessary. While with Dr. Ramsbotham I too would cherish "a perfect horror of all violent attempts at interference," I yet can see no valid objection to firm but cautious mechanical efforts to remove the placenta, when its removal is impossible by all other means, and when its presence is sapping the foundation of life, either by continued hæmorrhage or by poisoning the blood by its putrescent qualities. The danger of such mechanical efforts has, I think, been somewhat over-rated by the moderns, as will appear from a careful perusal of the older writers.

My attention was first specially drawn to this subject by a correspondence with the late celebrated Mr. Hey, of Leeds, of whom the French speak as the patriarch of modern English surgery. In my letter to him, which is dated January 3rd, 1819, on the subject of an

* Read at the general meeting of the East York and North Lincoln Branch.