

any further notice of his observations or entering into any discussion with him on the subject. With regard to Dr. Clinton, whom he endeavours to identify with himself in his views and statements, I confess the only way in which I can reconcile to myself the idea that he ever sanctioned the publication of such a paper, supported by his name (if he really did so), is that he unthinkingly entrusted himself in the hands of the writer of this paper, and acquiesced in his views without taking upon himself to inquire into the subject, and certainly without having read my paper which his name has been brought forward to overthrow, as, unless my opinion of this gentleman shall become altered, I cannot bring myself to imagine that he would wilfully have countenanced so much misrepresentation and ignorance.

I am, Sir,

Your obedient servant,

EVORY KENNEDY.

Lying-In Hospital, Dublin.

ON THE USE OF

THE STETHOSCOPE

IN THE DETECTION OF PREGNANCY, &c.

By DAVID C. E. NAGLE, A.M., M.B.,
Trinity College, Dublin.

(Concluded from page 400.)

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AWARE of the almost unlimited degree of confidence which medical men are apt to repose in the opinions of Laennec, I apprehend that his arguments on the controverted question, "What is the site of the soufflet?" will be deemed by many entitled to very respectful consideration, notwithstanding his want of experience in the study of the phenomena afforded by gestation. To those arguments I shall now take leave to direct the attention of the reader; and whilst I am endeavouring to point out their fallacy and insufficiency, I shall at the same time be submitting to the profession my own views of the matter, without, however, expecting more attention to them than the proofs I may adduce will warrant the practitioner in considering them entitled to.

Laennec scientifically reduces the question into the form of a disjunctive proposition, which, however, he does not render sufficiently adequate or comprehensive. He is, besides, infelicitous in his mode of arguing, from the remotion of all the parts but one to the position of that one. This infelicity we must attribute to his inexperience in the study of obstetric auscultation, and not, by any means, to either a deficiency of talent or a want of candour; for his mas-

ter mind could never deliberately condescend to resort to sophistry in his laudable efforts for the establishment of so grand and useful a principle in diagnosis. "The only arteries," he says, "in which the sound in question can be supposed to be produced, are the hypogastric, iliac, and uterine; if the two first were the site of it, we ought to hear it on both sides of the uterus at once, or alternately, which is not the case."

Now, with all possible deference for his opinion, I have proved, and I hope satisfactorily, that it is the case—that we do hear it on both sides of the uterus at once, or alternately; and of this any one, who has the tact of examining adequately, can easily satisfy himself. I have, indeed, seldom failed in finding it on both sides at once, exactly in front of the superior anterior spinous process of the ilium, opposite which, nearly, the uterine arteries are given off by the internal iliacs. I would take the liberty of putting the argument thus. If the two first were the site of the murmur, we ought to hear it on both sides at once, or alternately: but we can so hear it; and I therefore respectfully submit, that we are warranted, even by his own mode of reasoning, to conclude, that the two first may be the site of the soufflet. The legitimacy, at least, of this inference, no one, I believe, will be disposed to question. The next part of his disjunctive proposition he thus expresses:—"If all the uterine arteries yield it, we ought then to hear it in different parts, and several at the same time." As he does not conclude the argument, I shall endeavour to do so; and, I think, it will be fairly expressed in the following manner. If all the uterine arteries yield it, we ought then to hear it at once over every part of the uterus: but I am borne out by experience when I assert, that we cannot, at any time, hear it over every part of the uterus at once; and, therefore, it may be fairly inferred, that *all* the uterine arteries cannot be the site of the murmur. Laennec comes to the following conclusion:—"What seems to me most probable is, that it exists in the chief artery distributed to the placenta." The incorrectness of this opinion I shall endeavour to prove by the following mode of stating my objections.

If the soufflet exists in that part alone of the chief artery which is distributed to the placenta, we can have it only where there is, or lately has been, a placenta. But Corrigan's case proves the certainty of its existence where there is, or lately has been, no placenta; and, therefore, I feel that I am justified in drawing this inference, that it does not exist in *that* part of the artery which is distributed to the placenta. Now, let us consider if it can have its proper site in the *trunk* of that vessel. Well, if it

exists there, we should hear it over the course of the trunk of that artery; but I imagine I have fully demonstrated that it takes exactly that course from below, upward and inward, towards the median line; and surely I may, without incurring censure, feel myself justified in asserting the possibility, nay certainty, of its existence in that part only of the artery. Besides, when the soufflet is at all discoverable, I never yet was disappointed in finding it over the point nearly where the lateral uterine artery takes its origin from the internal iliac; and I feel that I am not hazarding a rash opinion when I submit, that we can detect it when there is no placenta, if the uterus be enlarged by disease; for we know that the chief uterine arteries are greatly distended, not only during pregnancy, but whenever the size of the uterus is much increased by any morbid condition. Another proof of its existence in this part of the vessel chiefly, is afforded by the fact, that whenever we examine for it the lower part of that artery, the murmur is found to be confined to a narrow, but not to a short space, and gives the idea of its proceeding from a LARGE vessel; but as we move the cylinder upward and forward, it gradually becomes more diffused, as the trunk gives off its first large branches, and insensibly dies away towards the commencement of the vessels with narrow calibres, that is, towards the points of anastomosis, with the corresponding branches of the opposite side. I have no doubt that the soufflet may exist in the external iliacs also; for I have repeatedly traced it "from a point a little above the superior anterior spine of the ilium in a line, taking, from within outward, directly the course of the external iliac, even to Poupart's ligament; and, in the upper part, affording the perception of a sound deeply seated, but gradually becoming more superficial as we approach the ligament. Moreover, we can recognise the murmur to be produced by a vessel of large calibre, which could never be the case if it were confined to the vessels alone which run into the placenta. I feel that I shall not be presuming too far, in thinking that part of Dr. Kennedy's paper not perfectly correct, which supposes that change of position will, during the gravid state of the uterus, remove all pressure on the bifurcating parts of the common, or, at least, internal iliacs, and those branches of the middle hæmorrhoidal, which are given off to the lower part of the uterus. And experience ought to teach us, that the resonance will extend a considerable way from the point of obstruction, no matter of what nature the tumour may be which would press upon these vessels; I therefore put it to the judgment of every rational practitioner, whether we are justi-

fied in pronouncing a female pregnant, merely because we hear in the pelvic, iliac, or lumbar regions, a distinct or prolonged murmur. Certainly not, if what I have been stating be at all entitled to any credit.

The impossibility of the murmur being seated either in the comparatively small vessels which run INTO the placenta, or in those that pass through the parietes of the uterus UNDER the placenta, as Drs. Kergaradec, Kennedy, and others, would have it; and the certainty of its having its site in the large vessels, more especially in the enlarged trunk of the lateral uterine arteries, fully justify the inference that the soufflet is not liable to be affected in its QUALITY by the life or death of the fœtus in utero, as Dr. Kennedy would have us to conclude. This being a question of paramount importance, and one in which I happen to be diametrically opposed to his view of it, I shall now proceed to the consideration of the subject; and that I may the better enable the reader to form his own opinion, I shall endeavour to lay before him, as succinctly but as fairly as possible, Dr. Kennedy's sentiments on the point at issue.

In the last volume (5th) of "the Dublin Hospital Reports," page 267, he states, "another advantage of importance we derive from the placental sound, is its assisting us in pronouncing on the life or death of a fœtus in utero." And again, p. 269, "it affords us an indication of the death of the child, viz. either by ceasing entirely after having been previously heard, or having its character altered from the continuous murmur, with its lengthy sibilous termination, to an abrupt, defined, and much shorter sound." In opposition to this, I can assert with a confidence not over-weening, but, I presume, not ill founded, for I derive it from considerable experience, that neither change NECESSARILY follows on the death of the fœtus in utero. The murmur, which we most frequently meet with when the child is alive, is not the "continuous one with its lengthy sibilous termination;" and even when the child is dead for weeks, we can hear the same description of murmur we usually meet with when the fœtal pulsations are most energetic. So it was in the case examined, as I mentioned, by Surgeon Robinson and myself.

In another part, p. 247, Dr. Kennedy says, "The circulation in the mother and maternal part of the placenta being independent of that of the fœtus, we can understand how a phenomenon produced by the former should exist when the latter has ceased. From this we might be led to expect, that the sound should exhibit the same characters, whether the fœtus be dead or alive; but in doing so, we should fall into error." With great deference for Dr.

Kennedy's opinion, I really cannot avoid feeling that he does not adduce a single good or satisfactory argument in proof, that "by doing so we should fall into error." He gives, it is true, a case, p. 246, in which the funis, he was INFORMED, had protruded an hour before his visit; the pulsations in it were observable at the time of its protrusion, but ceased shortly after. No fœtal heart could be heard by him, but the placental sound was, however, distinctly perceptible; "it was full but shorter, more abrupt in its termination, and wanting the sibilous whiz, characteristic of the perfect utero-placental circulation. The incongruity of all this must strike the least observant. He first gives it as his opinion that the maternal and fœtal circulations are QUITE independent of each other; and because, in the case alluded to, the fœtus happened to be dead for certainly not more than half an hour, the maternal circulation should, indeed, be so strangely altered in that short space of time, as to change altogether the character of the soufflet. But Dr. Kennedy, when adducing this sort of case in support of his doctrine, never reflected that he knew not the character of the murmur previous to the death of the child. He did not examine it; and because he, on his examination of it when the child was dead, found it to have a particular character, it must therefore, of necessity! have had a different one at a time when he had no opportunity of ascertaining whether it had or not—"credat judæus apella." By such an ingenious mode of reasoning, he would certainly be going far towards establishing the validity of the "post hoc, ergo propter hoc" mode of reasoning; but I imagine it will not gain over to his opinion many converts from among the intelligent, such as I am gratified to find, my "native land" can at present boast of in the several departments of the medical profession. In opposition to the inference he would have us draw from such a description of case, I have given one p. 398, where the child was supposed to have been dead for three weeks, and the placenta was described to have been quite small and impoverished; yet Mr. Robinson and I detected a perfect soufflet, such as we ordinarily hear when the fœtal circulation is most perfect; and, perhaps, after much pains-taking industry, it would not be arrogating too much for either of us to say, that we could not be deceived in a matter which really was attended with no great difficulty.

From the case he gives, p. 246, Dr. Kennedy deduces an inference, in the validity of which I at least cannot concur, namely, that "to produce the perfect soufflet, it is necessary the blood should also traverse the placenta itself." By this he would induce us to suppose that, because in the case he

presented to his readers, he found the child to be dead for scarcely more than half an hour, the maternal circulation must necessarily have ceased in the placenta; whereas he admits that circulation to have no connexion whatever with that of the fœtus. Space will not permit me to follow Dr. Kennedy through the several statements he makes; but I incline to think that the very best refutation of his doctrine will be found in his own paper, which, I regret to think, will scarcely stand the test of serious examination.

As it strikes me, and I say it without meaning the slightest offence, he mistakes altogether the principle on which the QUALITY of the soufflet depends. He supposes its character to be determined by the circulation through the placenta of the maternal blood, modified by the life or death of the fœtus. I would respectfully submit that the character of the soufflet depends exclusively on the *quality* of the maternal circulation, such as the strength, quickness, or slowness of the pulse, and on the diameters of the conduits through which the blood has to pass; and, consequently, that it has no necessary dependence on the life or death of the fœtus in utero, and, therefore, not to be taken in any case as a sign for us to form our diagnosis by. No one will deny that the murmur is perfectly synchronous with the maternal pulse. When the pulse is quick and weak, the natural murmur will be short or abrupt, as it coincides with the interval between each two successive pulsations, always commencing with the incipient state of each beat at the wrist. Therefore, a short or "abrupt" murmur, essentially depending on such a principle, should not be considered as a criterion indicative of the vitality or non-vitality of a fœtus in utero; for it does not necessarily follow, because the fœtus is dead, that the maternal circulation must invariably be quick. It sometimes happens that even a quick pulse, if it be strong, will produce the "continuous murmur," without this having any, the least, connexion with the vitality of the fœtus.

When the maternal pulse is slow, and not very strong, the murmur will, in general, be lengthened, loud at the commencement, and gradually decreasing towards its end, or the beginning of the next pulsation. Even when the interval between each two consecutive pulsations at the wrist of the mother is long, the murmur may be abrupt, particularly if the maternal circulation be not strong or excited. For instance, if I suppose the interval between each two consecutive pulsations to equal six seconds, the duration of the murmur may equal only three, four, or five seconds. In any of these cases, Dr. Kennedy would designate it "abrupt," be-

cause it did not exactly continue for the six seconds, or, in other words, coincide critically with the interval between each two consecutive pulsations. I felt the importance of paying to this soufflet an attention so particular and persevering, that my experience fully bears me out in the assertion, that we cannot, whether the fœtus be alive or dead, find it to retain any decidedly marked or permanent character, with reference to quality or duration. It is extremely variable. Whilst we are, during our examination, admiring, perhaps, the harmony and regularity of recurrence between each two consecutive murmurs, our admiration is often suddenly converted into a pleasing astonishment at the loudness and continuous intensity which they unexpectedly assume. It is not easy to account for those occasionally-increased murmurs in utero-gestation; but perhaps we should not be far from the truth, by attributing them to the streams of blood endeavouring to force their way through their wonted channels, of which the diameters may be at times a little more than usually decreased by various causes, even by the fœtus assuming a new and convenient position in the womb; or, independent of the latter, to a moral excitement in the mother, giving an increased momentary impulse to each successive column of her blood.

But why do I dwell upon this murmur? simply, because I conceive it to be a sign of paramount importance to the discriminating physician in forming his diagnosis. Though I cannot bring myself to consider it an unequivocal sign of pregnancy, I am ready to admit it as perhaps the least equivocal of the equivocal ones; and its existence, taken in conjunction with the history of the case, is calculated to raise in the mind of the reflecting practitioner a strong suspicion, at least, of impregnation. Our attention being directed to the character of this murmur, we shall be able to infer how fallacious is that theory, which would have us suppose that the quality of the soufflet should be taken as an indication of the life or death of the fœtus. This soufflet Dr. Kennedy supposes to be produced, either by the blood passing through the arteries of that part of the uterus to which the placenta is attached, without passing into the placenta itself; or "that it may greatly depend on the passage of blood through those uterine vessels which pass into the maternal portion of the placenta." In the first case, the sound would be occasioned merely by the pressure of the placenta on the vessels. Now, if this were the cause of the murmur, which I deny, how could the death of the fœtus so affect it, as to produce, all at once, so important a change in its character?—unless he supposes that dead matter becomes im-

mediately far lighter than living matter; and, therefore, that the former weight upon the placenta, being now necessarily diminished by the death of the child, the pressure previously made upon the arteries running under the placenta, must also be decreased. But, on the other hand, if the murmur, according to him, "may greatly depend on the passage of blood through those uterine vessels which pass into the maternal portion of the placenta," I would venture to say that, even so, the death of the child could not induce such an instantaneous change in the quality of the soufflet, if, as he admits, the two circulations are perfectly independent of each other. I beg it will be considered that I mean this latter argument only as an "*argumentum ad hominem*;" for I cannot agree in opinion with some others, that the two circulations are totally independent of each other; that they are connected by absorbents at least, I am scarcely wrong in supposing; and on this account I think it a very fair inference to consider, that when the fetal circulation has ceased for some time, the circulation in the maternal portion of the placenta should also undergo some alteration, and consequently the murmur, if "it depend greatly on that circulation," exhibit, in like manner, some modification. But experience has fully proved to me, at least, that it does not undergo the slightest alteration in quality; and I, therefore, take it as another strong proof that the soufflet is not owing to the "passage of blood through the chief artery distributed to the placenta;" and also that its character is not, necessarily, liable to be affected by the death of the fœtus in utero.

I should, indeed, be delighted if the profession could have so undeceptive a diagnostic in the character of this murmur; and with the view of ascertaining this important point, I had frequently, before Dr. Kennedy's paper made its appearance, or I had any means of knowing his ideas on the subject, investigated the matter as critically as possible. The moment I heard of his views, my experience warranted me in denying totally the validity of his opinion; and I recollect to have told Dr. Kennedy, in the presence of some of the pupils of the hospital, that "there was in it at that moment a patient whose child was dead for some time, yet that he would find the soufflet prolonged and continuous." In support of the opinion which I am thus venturing to offer respecting the value of the soufflet as a diagnostic, I could adduce many cases, in addition to that described in p. 398 of *THE LANCET*; but I shall confine myself to one instance more, which, I am induced to think, will be quite decisive on the point. We had not very long since in the hospital a patient with a syphilitic taint; her child, auscultation

tion proved to be dead, and that this was the case for some time its excessively putrid state was well calculated to show. Yet in this case also, Dr. McEffer and Mr. Neville, both pupils in the hospital, were so satisfied of the existence of a full, prolonged, and, at times, continuous murmur, that they considered it a decisive corroboration of my opinion, in which I had the satisfaction of their concurrence on more occasions than one. In support of my view of this question, I might also adduce the testimony of some of my fellow-students at the Meath Hospital, where auscultation is carried to great perfection indeed, under the encouraging and judicious guidance of its eminently successful physicians, Drs. Graves and Stokes.

That auscultation should be deemed the only unequivocal sign of pregnancy, has been denied by some, apprehensive of placing, by such a concession, "their knowledge of practical midwifery in a very questionable shape." But in opposition to their doctrine, I not only am ready to concur in opinion with my respectable young countryman Dr. Ferguson, but willing to risk even my "knowledge of practical midwifery" on the hazard of the declaration, that auscultation supplies us with the only unequivocal sign of utero-gestation, in as far as we can detect by it the pulsations of the foetal heart, which banishes all doubt and gives our profession, in this instance, all the certainty of demonstration. What other unequivocal sign is there? Not a single one can any man even pretend to adduce. Here then the stethoscope supplies us with a paramount advantage; and I have no doubt, that, in any case where a foetal heart pulsates, the ear, which is sufficiently practised to accurate auscultation, will experience but little difficulty in its detection. Should the auscultator fail of hearing distinctly the pulsations themselves, their resonance, at least, will apprise him of the heart's existence; for even in cases where there was a very great accumulation of liquor amnii, the bare resonance of the pulsations enabled me to determine the precise point under which I could detect, most distinctly, the heart's action.

The inexperienced observer is liable, at times, to confound the pulsations of the foetal with those of the mother's heart, as the following case will not only prove, but also show the most satisfactory and obvious method of drawing the distinction. On the 9th instant, I was informed, in one of the "sick wards" of the hospital, by Surgeon H. Alcock, that there was in it a female in the seventh month of her pregnancy, and that he was informed the foetal heart was audible, but faintly so. In order to satisfy myself I had recourse to auscultation, and

heard below the umbilicus a feeble pulsation resembling, in some degree, that of a slow foetal heart; but immediately it struck me that it was not the action of an infant's heart. As I could not satisfactorily determine the point at once by a comparison with the mother's pulse, which was very rapid, I removed the cylinder to the præcordial region, when all doubt was instantly dissipated by the perfect identity of the rhythms. Should any difficulty arise to the inexperienced, in discriminating between the rhythms heard at such remote points, the observer has only to move the cylinder gradually from the lowest part of the abdomen, where the pulsations are detected, upwards towards the mother's chest, listening attentively during the ascent of the cylinder; and the slightest permanent discrepancy in the rhythms, determines that those in the abdomen are not produced by the action of the parent's heart, which, we know, can sometimes be heard as low down as the hypogastric region. The double beats, and the rapidity of the foetal heart's action, determine, in ordinary cases, the question without any difficulty, for in general they are not only double those of the mother's heart, but, in some instances, considerably more than double; as in the first of the twin cases given in a former paper in *THE LANCET*, where I mentioned that the pulsations in one foetus varied from 160 to 170, whilst those of the mother amounted only to 60 in the minute.

These are not the only advantages afforded by the stethoscope in the practice of midwifery. It further supplies us with the easiest and only means of ascertaining the presence of twins, as I have before pointed out; and experience authorises me to say, in opposition to any objections that may have been adduced by those unpractised in accurate stethoscopic observations, and who reason only from the CONVENIENT inspection of casts and plates, that we can in most cases determine by it the nature of the presentation. In two cases where the contrast was very striking, I have already shown the possibility of arriving at this marked and unquestionable advantage; and lately in a case where the kind of presentation was doubtful, auscultation alone enabled me to decide that it would be that of the breech. This advantage afforded by auscultation all must admit to be a most desirable improvement in the practice of midwifery; as, in addition to other benefits, we shall not, at any time, run the risk of rupturing membranes prematurely, and thus rendering dangerous, as well as tedious, the accouchement of our patient. Surely it is neither fair nor candid in men to argue from their own inexperience in the employment of the stethoscope to the incapability of

others more practised in its use ; or to conclude that, because one case of extreme difficulty may present itself, we should, therefore, despair of deriving any advantage from it in hundreds of others. I hope, for the sake of humanity, delicacy, and science, the reasoning of such *philosophic* and practical men will have but little weight with the judicious and unprejudiced portion of the profession.

If the great importance of the subject did not appear to me a sufficient justification for so lengthened a paper, I should feel myself called on to apologise for trespassing so much upon the attention of the profession. But the discussion, should it be productive of no other advantage, may, at least, be the means of inducing some persons, more competent and better supplied with the proper facilities, to rouse themselves from their inaction, and "let slip" inquiry for the discovery of the much useful information as yet acquirable in this department of our profession. To me, indeed, it is matter of regret, that, in the views I have taken, I should be under the necessity of differing so widely from the opinions of men pre-eminently distinguished. In doing so, I hope I have not transgressed the limits of legitimate discussion ; and to the unprejudiced portion of the profession I shall not only leave the decision respecting the questions in dispute, but to that decision I, at least, am ready to submit with the utmost deference and befitting respect.

33, Trinity College, Dublin,
Nov. 25th, 1830.

MEDICAL JURISPRUDENCE REDUCED TO THE CAPACITY OF A BAKER.

AN Attorney-Coroner, on being elected for a small district of a large county, applied to a neighbour in the medical profession to learn what work he should read, saying "that he supposed he ought to know a little of medical jurisprudence. The Doctor spoke of Paris's and Fonblanque's work. "Oh," replied the attorney-coroner, "I have seen that book, it is too deep for me ; it is deeper than Garrick." Beck's Elements were then mentioned as being more explanatory and easier of comprehension. "Ah, then," exclaimed the new-made Rhadamanthus, "*will you lend me the book for a few days ? ! ! !*"

THE LANCET.

London, Saturday, January 8, 1831.

THE period is not far distant when our medical, as well as our political, institutions, will feel the ameliorating influence of the intellectual revolution, which is now in progress from one end of Europe to the other. The voice of philosophy in France, the acclamations of triumph in Belgium, and the humble whine of concession to the stern dictates of necessity in broken-hearted Britain, proclaim the dissolution of systems in every department of life, whose existence was protracted, unfortunately, too long for the happiness of mankind.

To whatever point, indeed, of the social world we turn, the sight is gratified by the prospects of futurity, and the ear saluted with sounds of promise, which every movement of the mighty mass asserts the advent of a novel and a better era in the melancholy history of man. The various and rapid phenomena of the moral horizon, predict in short, the succession of an age of reason and intelligence, to an age of faith and credulity. There is, in fact, no mistaking these obvious signs of the times, and what is of nearly the same importance, their meaning may at length be promulgated without the fear of persecution. Principles which were silently entertained, or published with timidity by an order of men, whose superior genius placed them in the predicament of being born before the world was capable of benefiting by their speculations, have accumulated to such an overwhelming extent, and have become so generally diffused, that, from being enshrined in libraries open only to the eye of the curious, they have become the ordinary topics of conversation among the humblest classes of men, and the judicious regulators of their opinions.

We cannot, we conceive, render a more meet homage to this spirit of regeneration, whose slumbers we have sought to awaken from the first moment of our existence as public journalists, than that of pointing out the obstacles which so long impeded its progress in the medical profession. Already have we devoted, and not unprofitably we hope, much of our time and space to an ex-