

and 1831

DR. NAGLE ON OBSTETRIC AUSCULTATION.

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ON THE USE OF  
THE STETHOSCOPE  
FOR THE DETECTION OF  
PREGNANCY, A FÆTUS IN UTERO, &c.  
*By* DAVID C. E. NAGLE, A.M., M.B.,  
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“ Ου χρη παννυχιον εὔδειν βουληφορον  
ανδρα,  
ὧ λαοι τ' επιτετραφαται, και τοσσα  
μεμηλε.”

“ It had never occurred to me (said the illustrious Laennec) to apply auscultation to the phenomena of gestation. For this happy idea we are indebted to Dr. Kergaradec,

who obtained by it two results, which may now be considered as the most certain signs of pregnancy, namely, the pulsations of the fetal heart, and the simple blowing pulsation (*battement simple avec soufflet*), or placenta sound."

It may, perhaps, be considered presumption in me to differ in opinion from authority so weighty and influential with medical men as that which is supported by the names of Laennec and Kergaradec. But when I venture before the profession with views that, to many of its members, may appear both novel and untenable, I hope they will extend to me all the indulgence which should be awarded to one who really has no object in view but the advantage which may arise from a fair and candid examination of opinions and doctrines which appear to him to be erroneous. With much reluctance, indeed, should I attempt dissenting from any opinion offered by the talented Laennec, as the result of his *own* observation and experience; but when I reflect that, in midwifery at least, he must necessarily have had but a very limited experience, I feel sufficient justification for not fully coinciding with the inferences which he drew from Kergaradec's researches.

In a paper which I laid before the public in a late Number of *THE LANCET*, I considered auscultation to furnish us with the most and the only unequivocal sign of pregnancy, in so far as it enables us to hear the pulsations of the fetal heart, from the moment that it begins to act with any degree of energy; but I must be pardoned if I withhold my assent to the importance of the other phenomenon laid down by Kergaradec as a sure sign of utero-gestation. I perceive, with regret, indeed, that in the "*Dublin Medical Transactions*," lately published, Dr. Ferguson concurs in opinion with Kergaradec, that the "*placental bruit*," as the former designates it, should be considered as "*infallible evidence of a fetus in utero*;" and opposed as I am to such physicians as Laennec and Kergaradec, I hope my young and intelligent countryman, who himself admits that he has had but a very limited acquaintance with midwifery cases, will pardon me for a disagreement in opinion with him too, when I submit that we are not justified in placing any reliance on what is usually denominated the "*placental soufflet*," as an unequivocal sign of impregnation. In justification of this assertion, I trust I shall succeed in laying before the profession sufficient proofs and the most convincing facts.

During my attendance at the excellent Meath Hospital, I endeavoured by much industry and minute attention to the diseases of the chest, to familiarize my ear to the nicest distinction of the rales and sounds

afforded by disease. Prepared as I must thus have been for the detection of the stethoscopic phenomena presented by gestation, I entered, from my very commencement at the Lying-in Hospital, on the investigation and analysis of those phenomena. The result of some of my inquiries I shall now endeavour to submit to the profession.

I have already stated that the simple blowing arterial murmur, designated by some "*placental soufflet*," heard in the advanced stages of utero-gestation, should not be considered as an unquestionable sign of impregnation, and I trust I shall show by most satisfactory proofs that it is wrongly denominated *placental*, the placenta in my mind having nothing to do with its production. The contrary opinion I know is confidently maintained in a paper published in the last volume (5th) of the "*Dublin Hospital Reports*," by Dr. Kennedy, for whose understanding and industry I entertain so much respect, that I should be sorry even to insinuate that in his hands the stethoscope should be considered as an "*inutile lig-num*."

In the appendix to Laennec by Forbes, 2d edit., p. 703, it is stated that "*the bellows sound*" is usually heard "*on the side opposite to that on which the fetal pulsation is perceived; but this is by no means constant*." The latter part of this extract is, I humbly submit, the only portion of it deserving our attention; for in some hundreds of cases in which I carefully examined this phenomenon, I have, in ninety-nine instances out of every hundred, heard it as well on the one side as on the other in the same patient. It may, I admit, be masked on one side in some degree by the pulsations of the fetal heart; but an acute and practised ear will experience no great difficulty in detecting it even then. The great and unusual difficulty is, to find a case where it is really confined to one side. Whenever I happened to meet with any want of facility in detecting this sound during the day-time, I returned to the examination in the stillness of night, and generally heard it without much trouble, noting carefully that it was not that which proceeded from the opposite side.

When the patient is placed in the recumbent posture, with only a sheet interposed between the sternal extremity of the stethoscope and the abdomen, the auscultator will in most cases detect the *soufflet* at a point midway between the umbilicus and the superior anterior spinous process of the ilium, but not unfrequently closer to the latter. It often extends from this point towards the middle of Poupart's ligament, the loudness of the murmur in many cases increasing in a very marked degree as we descend; yet it not rarely assumes near the

gament a sharper character. From the same point it can often be traced upward and forward towards the mesial line, in the course, as it were, of the *trunk* of the lateral uterine arteries, which, it must be remembered, is enormously enlarged in the advanced stages of gestation. Thus then, in most cases, the soufflet can be traced from the middle of Poupart's ligament to a point midway between the scrobiculus cordis and the centre of a line extending from the anterior superior spinous process of the ilium to the umbilicus; and, in general, it is at the same time audible on both sides. Of this I have had indeed many satisfactory proofs; for instance, in the first of the two cases recorded in No. 376 of THE LANCET, in which case, while examining the patient, I pointed out the fact to Dr. Kennedy. To this circumstance respecting the sound I must beg leave to direct attention, while from the nature of the soufflet, and the extent of surface over which it could be heard, I endeavour to draw an argument against Dr. Kennedy's theory, that "the placental soufflet is heard indifferently over the abdomen," and that "it depends on the transmission of blood through the arteries of that part of the uterus to which the placenta is attached."

It will be recollected that in the above case of twins I stated that there was but one placenta. The patient having died, this was found to have been attached to the upper part of the fundus of the uterus, which, I also stated, was more than usually distended previous to delivery. Now, if the soufflet be owing to the attachment of the placenta in a case where it was thus affixed, it is very improbable that the soufflet could extend equally on both sides all the way down to the middle of Poupart's ligament, without gradually decreasing as we, in the descent of the cylinder, receded from the radiant point. Besides, when the cylinder was moved across any part of the abdomen from one side to the other, the soufflet could not be detected to extend *uninterruptedly*, even at the upper part of the uterus, as we might reasonably expect it to do, particularly when it was traceable down even to Poupart's ligament. We must therefore account upon other principles for the occurrence of the sound on both sides. Dr. Kennedy's expression, "heard indifferently," is so equivocal, that I am at a loss to determine the precise sense in which he meant to use it. If he means to imply, as I believe he does, that the sound can be heard, no matter what part of the abdomen we examine for it, the above case, and many others, will be directly opposed to him. In numerous cases I found the soufflet distinctly audible for a few square inches between the superior anterior spinous process

of the ilium and the umbilicus, but often approaching close to the latter, and it was even then quite perceptible in both iliac fossæ also. The question for us to determine in a case of this kind is, whether the sound on the opposite side is the effect of radiations from that part where the placenta *might* be supposed to have its attachment. Now the placenta was placed on that part, or it was not. If the placenta was placed on that spot, and thus gave rise to the soufflet on both sides, we might reasonably expect that this soufflet would radiate from one side to the other across the *anterior* surface of the uterus where the space must be narrower, rather than across the *posterior* wall where the greatest extension of the uterus is known to take place. Yet in no case was I ever able to trace it across the anterior surface of the abdominal parietes in an uninterrupted course, or even to detect it under the mesial line, except when it proceeded from the epigastric arteries, from which it can, in such a case, be easily proved to arise. But if the placenta was not situated on that part of the side of the uterus over which the murmur was so audibly heard, it will follow as a necessary consequence, that the murmur *there* must have originated in some other cause, an admission that would be fatal to Dr. Kennedy's theory, and the practical inferences he deduces from it. Now the cause of the murmur existing almost *invariably* in this spot, may, I conceive, be found greatly, nay chiefly, owing to the fact mentioned in his own paper, p. 239, that "in the neighbourhood of the ligaments, at the lateral parts of the uterus, we shall also find a *more full distribution of vessels*, even when the placenta is not attached to this part, as the principal vessels which connect the uterus with the maternal system pass into it *here*." To this fact I beg particular attention, as it is calculated very much to facilitate the settlement of the disputed question respecting the *site* of the murmur, and, consequently, whether, as we shall have occasion to discuss hereafter, the discreet and guarded practitioner would, without any other sign, be warranted by any change in the quality of that murmur alone, to pronounce on the life or death of a fœtus in utero.

Another position of Dr. Kennedy is, that when the soufflet is heard over the whole uterus, the placenta is then attached to the anterior wall of the organ. It will be easy, I think, to prove that this inference is unfairly deduced. In such a case the soufflet would be more distinctly audible, in proportion as we approached in our examination to the point of insertion, whereas the contrary is the fact; for the more we recede from the mesial line towards the iliac fossæ, the clearer, as far as my experience at least



warrants the assertion, does the murmur invariably become. Now let me suppose that the placenta is situated under the mesial line, what should we expect? Why, that the soufflet would be most distinctly audible over a space coinciding with the diameter of the placenta, and become fainter and fainter as we receded from that point of radiation. But I have already shown that the contrary is the fact. The same mode of reasoning will apply if the placenta be situated on the fundus of the uterus, for the murmur is generally heard louder at a point which is nearer to the pelves than to the fundus of a distended uterus.

In order to meet some assertions of the French writers, it will be necessary to refer to page 703-4 of Laennec. There he says, "What seems to me most probable is, that the sound in question exists in the chief artery distributed to the placenta," and then alludes to a communication made to him by Dr. Ollivry, who is represented to have expressed himself to the following effect:—"The point where I had previously heard the blowing pulsations, corresponded exactly with the point in which the placenta was implanted;" and again; "A proof that the cause is what you have stated, is found in the fact that the sound *ceases* the very moment the umbilical chord is cut." In his opinion a very triumphant, but in mine "a very lame and impotent, conclusion." As Laennec has himself successfully ridiculed the "*post hoc, ergo propter hoc*" mode of argument, perhaps I shall be fortunate enough to meet forgiveness from some of those distinguished men whose doctrines I am thus presuming to impugn, if I submit that Ollivry's "*prope hoc, ergo propter hoc*" is equally inconclusive. The admirers of Drs. Laennec, Kergardec, Ollivry, Ferguson, and Kennedy, will be startled, perhaps, when I assert that the "self-same" identical description of murmur or soufflet, which usually occurs in the advanced stages of pregnancy, is distinctly presented to the ear, when there is no foetal circulation at all going on—where there is or has been *no placenta*! And now for the proofs.

In the first place I shall venture to assert, that the foetal circulation has nothing to do in the production of the murmur in question; that it can and does exist with its characters unaltered, even when that circulation is destroyed, no matter for what length of time; and therefore that we are to attach no importance either to the soufflet, as an infallible test of gestation, or to Ollivry's assertion, that "the murmur ceases the very moment the chord is cut." I could adduce many cases in support of my assertion, but the following will, I hope, appear sufficiently decisive.

On the 27th ultimo, a patient was admit-

ted into the Lying-in Hospital with abortion threatening, in consequence of ill usage received about three weeks previous. My acute and intelligent friend, Surgeon R. Robinson, was engaged in examining, with the stethoscope, this woman when I entered the ward. He expressed a wish that I should examine the case, observing to me, that he could not hear the foetal heart, but could distinctly perceive, in the right iliac fossa, a murmur *prolonged*, and not by any means "*abrupt*;" "but if I am (said he) to be influenced in my diagnosis by the theory of Dr. Kennedy (the truth of which he knew me all along to deny in the most decided terms), I must, from the distinctness and prolonged nature of the murmur, conclude that the foetal circulation is still going on." I examined the patient, could detect no foetal pulsation, but heard, on the right side, the murmur as described; it was also audible in the left iliac fossa, but weaker than on the opposite side. Convinced that by this case too I should be furnished with a powerful argument against Ollivry and against Dr. Kennedy's theory, respecting the quality of the murmur being a test indicative of the life or death of a foetus in utero, I remained in the ward until about four o'clock that day, at which hour the patient was delivered of a foetus, very small, dead, and so putrid, that not only had the funis been divided, as it would appear, for some considerable time previously, but, as it was a breech presentation and the parts in a very undilated state, I had considerable difficulty during the extraction of the foetus to prevent its limbs from falling asunder. The patient was in about the seventh month of her pregnancy; had received, about *three* weeks before her delivery, an injury on the side (after which occurrence she did not feel the foetus to move in utero), and the suspicion of its having been three weeks dead was fully justified by its excessive putridity. It is important to observe, that the placenta, in this case, was very much impoverished, and its diameter not greater than that of the palm of an adult's hand. This case, then, proves, not only that the murmur is quite independent of the foetal circulation, but that persons, unaccustomed to accurate stethoscopic observations, would, if influenced by Dr. Kennedy's theory respecting the quality of the soufflet affording a sure indication of the life or death of a foetus in utero, be liable to fall into very serious and egregious errors, as, from the practical importance of the fact, I shall have to prove more fully in the subsequent part of this paper.

I shall now proceed in my endeavours to show, that the presence of a placenta is not necessary for the production of a murmur, such as we ordinarily hear in the advanced stages of gestation; and that we can detect

it, as in certain cases of disease, when there is, or has been, no placenta at all. Here I am perfectly at issue with Drs. Ferguson and Kennedy, who assume that the soufflet should be considered as a test of pregnancy. As subversive of such dangerous theory, I am happy to have it in my power to instance the following case, which Dr. Montgomery, Professor of Midwifery to the King and Queen's College of Physicians in Ireland, did me the favour of taking me to examine on the 18th inst., at Sir P. Dunn's Hospital, into which the patient, Ellen Corrigan, aged 40, was admitted about three months before. She had had but one child, now twelve years old, after whose birth the catamenia continued regular until about four years ago, when she was attacked with fever, after which they began to exhibit some irregularity in their quality and the period of recurrence. Immediately after the fever, she began to suffer from weakness and sickness of the stomach which lasted six months, when, for the first time, she observed in the left iliac fossa a tumour, a hen's egg in size. This tumour repeatedly produced a lancinating pain that would frequently dart across the abdomen to the opposite side, to which, with a convulsive effort, she would apply the hand to arrest as it were the pain, and grasp the tumour that, she fancied, had shot across from its usual situation. The menses, she states, continue pretty regular and natural; the tumour is subject to great variety in size, and at present exhibits many of the characters observable on the abdomen of a woman in the seventh month of her pregnancy, and indeed the female has often been suspected of being pregnant. The right lower extremity is frequently more swollen than the left, and the veins are described to have been in a very enlarged condition. Such is the case I had to examine for the "placental soufflet" of authors; and in the right iliac fossa I detected, in the presence of Dr. Montgomery, an intense and lengthened murmur, which he also heard, and which, when the patient lay in the horizontal posture, I found to proceed from a point near the anterior superior spinous process of the ilium, upward and forward, towards the mesial line, as in cases of pregnancy. I then made the patient turn quite on the right side, so as to lessen, as far as can be done, the pressure on the left iliac vessels; yet the sound could be heard here, even in such a posture, without any material change in its character; so in like manner did I examine the right side, where the soufflet was invariably more intense than on the left. When she got into the erect posture, the murmur continued unaltered on the right, but became a little weaker on the left side. It is unnecessary to say that I examined with particular care this case, so

interesting, as far as the stethoscope, at least, is concerned. It was mentioned that the right lower extremity was far more swollen than the left, and that the murmur on the right side was louder than on the left. To a reflecting mind may it not appear, that these two circumstances might bear the relation of cause and effect to one and the same thing—increased pressure on the right iliac vessels, and, by a necessary consequence, an obstruction to the free transmission of blood through them?

To an unprejudiced person I would put the case thus. If the soufflet in question is to be considered an *infallible* proof of pregnancy, it can exist only where there is or has been a placenta. But I may be permitted to hope that the above case, of nearly four years' standing, fully justifies the belief, that we can have this murmur when there is no placenta. I therefore respectfully submit, that we should no longer deem it an infallible test of utero-gestation. Again; if the murmur depend on the presence of a placenta, it is only fair to infer, that its intensity, and the extent of surface over which it can be heard, ought to be in proportion to the size of the placenta; but I shall show that this proportion does not exist, and therefore the conclusion to which we ought to come must strike every unbiassed mind. First; in the second of the twin cases, which I lately laid before the profession, there were *two* placentæ, each of the ordinary size; yet I could not, by the most minute examination, detect, previous to birth, any alteration in the character of the soufflet. Again; on the 17th of September last, a female was delivered in the hospital of a healthy fœtus, whose umbilical chord, of the ordinary size and length, bifurcated within three inches of its termination in the placenta, and each branch was inserted into a distinct placenta of the usual size and consistence; yet, even in this remarkable case, there was no unusual variety observable in the nature of the murmur. Thirdly, in the case where, as I mentioned, there was a very small and impoverished placenta, Mr. Robinson and I detected a loud and lengthened murmur.

Having thus far endeavoured, for the sake of truth, to combat the ingenious theory of Dr. Kennedy, Ollivry, and others, I feel that I cannot, with propriety, decline offering some observations respecting my own opinion about the *site* of the murmur; this I shall venture to do, and shall take it as a particular favour, if I am in error, that my mistake should be rectified by some more experienced and more intelligent member of the profession. In the opinion which I am induced to adopt, I have many to agree with me; and I own it is to me both flattering and encouraging to find, that my

view fully coincides with that of a highly respectable fellow of the King and Queen's College of Physicians in Ireland, my very talented and very estimable friend Dr. Clinton.

[We defer to a subsequent Number the remainder of Dr. Nagle's observations.—  
Ed. L.]

## OBSTETRIC AUSCULTATION.

*Reply of DR. KENNEDY to DR. NAGLE.*

*To the Editor of THE LANCET.*

SIR,—I confess I felt somewhat surprised on reading a paper in your publication of the 18th inst., on obstetric auscultation, purporting to be written by a gentleman signing himself David C. E. Nagle, A.M., M.D., T.C.D. How this gentleman could have so grossly (I regret being obliged to use the expression) misrepresented the observations and facts set forth in a paper which I some time since published in the fifth volume of the Dublin Hospital Reports, I am at a loss to conceive. That his doing so could not, however, have arisen from his not understanding me is, I fear, but too evident; as it certainly required more ingenuity to misconstrue, and more art to pervert facts and statements as he has done, than could possibly be necessary to comprehend them. I should therefore deem a lengthened refutation of his paper not only unnecessary, but unbecoming, and shall merely point out a few of his most palpable misstatements, and beg to refer those who may feel interested in the subject, to the paper which I have already published. He asserts (pages 396 and 399) that I, with Dr. Ferguson, assume the "dangerous theory" that the placental sound should be considered an unquestionable test of impregnation; this I deny, these are my expressions:—"The placental soufflet (taken in conjunction with the other symptoms of pregnancy) will tend much to strengthen the opinion of the presence of this state. The co-existence of the fetal pulsation with the soufflet of course decides the question, but with regard to the latter taken separately, I will go so far as to say, that were all the other symptoms of pregnancy absent, and that this could be distinctly perceived, I should at least withhold my opinion until a sufficient time had elapsed to place the matter beyond a doubt." See Dublin Hos-



pital Reports, vol. v., page 258-9; see also pages 255 and 257, where such "dangerous theory" is not only disclaimed but actually condemned. Again he advances, page 398, "That persons, if influenced by my theory, as he styles it, respecting the quality of the *soufflet* affording a sure indication of the life or death of a fœtus 'in utero,' would be liable to fall into very serious and egregious errors." That I support any such theory or hold any such opinion as this, I must again in the most unqualified terms deny; that I am fully justified in doing so, will appear from the following, which are my words, when treating of this alteration in the character of the sound, Hospital Reports, page 269:—"The placental sound, either by ceasing altogether 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, together with the impossibility of detecting the fetal heart's action, particularly if such has been before observed, places the child's death beyond a doubt." Why was the concluding portion of this quotation so cautiously, so unfairly suppressed? It was that he might give a "case" in which, though the fœtus was putrid, he heard a murmur, *prolonged* and by no means *abrupt*; evidently insinuating that I deny the possibility of the occurrence of such a case; and he adduces it triumphantly, and "as furnishing him with a powerful argument against the quality of the murmur being a test indicative of the life or death of the fœtus in utero." Here is the "suppression verbi" coupled, as it usually is, with the "assertio falsi," for I have given a case (page 250) where the sound was not "abrupt," although the fœtus was dead, and I also mention having observed such, where the fœtus exhibited marks of having been dead for weeks. With these facts staring him in the face, he asserts, with what justice I leave to you and your readers to determine, that I consider the *quality of the soufflet as affording a sure indication of the life or death of the fœtus in utero*. With regard to his discovery (page 398) that a placenta is not necessary for the production of murmur such as we ordinarily hear in the advanced stages of utero-gestation, and the inference he would draw of my inaccuracy, I beg to refer the reader to a fact, of which, however it may suit Mr. Nagle's views to appear so, he can scarcely be ignorant, namely, that I have already stated, without any disguise (page 266), "a case where a sound resembling the soufflet from a morbid cause (a considerably enlarged liver) was observable;" and also mentioned (page 265) that those unacquainted with the stethoscope may be deceived by other

sounds, from their resemblance to the placental soufflet, several of which I instanced. So much for misstatements and suppressions with regard to myself. I shall now notice, very briefly, a few of those general statements which are so palpably opposed to fact, that I should deem myself culpable in passing them over without some remark. He says that the "bellows sound (or soufflet) is heard in ninety-nine cases out of every hundred, as well on one side as on the other, in the same patient;" now, although I admit, and have mentioned the fact of its being occasionally heard on both sides in the same patient, I have no hesitation in stating as far as my experience goes, the above proportion is most enormously exaggerated. It (the bellows sound) may, I admit, says he, "be masked in some degree by the pulsations of the fœtal heart." Ridiculous! Can the fœtal pulsation "mask" a sound at least ten times louder than itself? The assertion "that he was never able to trace it (the soufflet) across the anterior surface of the abdominal parietes in an uninterrupted course," may be perfectly true, but if he means that because *he cannot hear it*, it cannot be heard there by others, I set it down as of a piece with those already mentioned, and I doubt whether he is really capable of recognising the phenomenon of which he treats. "Or ever," he adds, "to detect it under the mesial line, except when it arose from the 'epigastric arteries.'" What degree of credit will be attached to the reasonings of a person who displays such ignorance of anatomy? I will venture to say that the merest tyro at the profession could have taught him that in no case do the "epigastric arteries lie under the mesial line;" and that in the abdomen, distended as it is in utero-gestation, each of these arteries lies from four to six inches distant from this line. Again, he speaks of the "sound being traced upwards and forwards towards the mesial line, in the course, as it were, of the trunks of the lateral uterine arteries." The trunk of the lateral uterine artery upwards and forwards and towards the mesial line!! This is indeed "as it were," but not "as it is." If this gentleman had rested satisfied, merely with adopting views and opinions grounded on such data, however incorrect and absurd, and had his positions been supported by inaccuracies and discrepancies more palpably striking than his paper exhibits throughout; it should have remained uncommented on by me, as such productions generally find their own level. I would, however, and with justice, have been to blame as well upon my own account as on that of the profession generally, had I allowed such a tissue of misrepresentation to remain uncontradicted. Having now done so, I must decline taking



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,

EVERY 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 remoteness 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 justifi-

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. Kergardec, 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, defused, 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 foetal 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 foetal circulations are quite independent of each other; and because, in the case alluded to, the fetus 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 foetal 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 fetus. 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 fetus. 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 fetus 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 fetus in utero; for it does not necessarily follow, because the fetus 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 fetus.

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, ausculta-



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 fetal 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 fetal 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 fetal 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 fetal 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 fetal 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 fetal 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 fetus 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.

### OBSTETRIC AUSCULTATION.

LETTER FROM DR. CLINTON ON THE SUBJECT  
OF DR. KENNEDY'S REPLY TO DR. NAGLE.

*To the Editor of THE LANCET.*

SIR,—In Dr. Kennedy's reply to Dr. Nagle's paper on obstetric auscultation, certain charges are preferred against me, which are neither founded in fact, nor, as it appears to me, in the slightest degree warranted by that passage in Dr. Nagle's paper, which, as far as I know, furnishes the only ground for the accusation. In Dr. Kennedy's reply, it is insinuated that I have unthinkingly intrusted myself into the hands of Dr. Nagle; that I have acquiesced in his views without the trouble of inquiry; that I have not read Dr. Kennedy's paper, which my name is brought forward to overthrow; and,

lastly, that I have wilfully countenanced misrepresentation and ignorance. If it be a fact, as Dr. Nagle states, that I agree with him in opinion, Dr. Kennedy seems to argue that the truth of these charges must inevitably follow. It will be my business to show, that my own innocence of the above charges, and Dr. Nagle's veracity, are quite compatible with each other.

It will appear, on referring to Dr. Nagle's paper, that the only one of his views and statements in which he says that I coincide with him, is that which relates to the site of the soufflet. It is, therefore, unwarrantable to impute to Dr. Nagle the desire of identifying himself with me in all his views, and it is perfectly gratuitous to assert that the one opinion in which Dr. Nagle says we agree, was adopted by me on his authority, without previous examination. The contrary is the fact. I have long entertained the opinion, that the sound which is called the placental murmur is not seated in the placenta itself, but in the large arteries around the uterus. In this opinion I have been confirmed by the facts and arguments contained in Dr. Nagle's paper; for he was so kind as to communicate them to me, when he was making that point the subject of particular investigation. He was therefore perfectly correct in stating that I agreed with him in opinion respecting the site of the soufflet. But it does not by any means follow, that the charges brought against me by Dr. Kennedy are true; for it appears that there is no ground whatever in Dr. Nagle's paper for the charge of my adopting ALL his views, as that paper mentions only one instance of an agreement in opinion between us, nor any ground for the insinuation that such opinion was received by me without inquiry, as it, in fact, was previously entertained by myself. It is true, however, that the opinion in question was formed without consulting Dr. K.'s paper, and what perhaps will surprise Dr. Kennedy much more, it is equally true that it still remains the same, although I have since considered it with the advantage of all the lights afforded by Dr. Kennedy's able production. Perhaps the knowledge of this circumstance will induce Dr. Kennedy to change his opinion respecting the facility with which I may be led to adopt the views of others without due examination. It is not very probable that the opinions of a man who could resist the force of his arguments would yield to those of another, whom Dr. Kennedy considers very ignorant; nor is it likely that such a man would *unthinkingly* intrust himself into the hands of any writer, even though it were Dr. Kennedy himself.

As to the supposition that my name was brought forward to overthrow Dr. Kennedy's paper, it scarcely deserves a serious refuta-

tion. It can hardly be imagined that Dr. Nagle could expect to derive any support to his opinions, from the authority of a man who was not known to have paid any attention to the subject in dispute between him and Dr. Kennedy. Besides, Dr. Nagle is well aware that facts and arguments alone can decide a disputed question in science, and that the authority of any individual, however respectable, scarcely deserves any consideration. I therefore ascribe the use which he has made of my name to a very different motive, to a desire of making it known that I was not inattentive to a branch of medical science, which all those who know me might suppose to be wholly neglected by me. To me it is a matter of wonder, that those who know Dr. Nagle, who is one of the most accurate auscultators with whom I am acquainted, should suppose he could derive any advantage from appealing to my authority in support of his particular views regarding obstetric auscultation.

It is obvious that, as I had not read Dr. Kennedy's paper before Dr. Nagle's was published, I could not be a party to the *alleged* misrepresentations contained in the latter; and this I hope will preserve me from the calamity of forfeiting Dr. Kennedy's good opinion, which I shall be very happy to retain as long as I can do so, without the hazard of losing my own.

It will also be granted that, as I have now attempted to correct the *unintentional* misrepresentations of Dr. Kennedy, it is not very probable that I would sanction the *wilful* misrepresentations of Dr. Nagle, if I knew of any such. It appears, however, that as far as I am concerned, the charge of misrepresentation against the latter is quite unfounded; and I may now add that I do not know any man who is less capable of wilfully misrepresenting another than Dr. Nagle, and least of all a gentleman for whom he entertained a high respect, as I know he did for Dr. Kennedy, at the time of publishing his first letter.

I am, Sir,  
Your obedient humble servant,  
P. CLINTON.

Dublin, Jan. 21, 1831.

#### REPLY OF DR. NAGLE TO DR. KENNEDY.

“Neque ego illi detrahere ausim,  
‘Hæretet’ capiti ‘si ulla’ cum laude, coronam.”

#### To the Editor of THE LANCET.

SIR,—If in his letter, published in THE LANCET of the 8th inst., Dr. Kennedy had confined himself to facts and arguments, instead of resorting to intemperate and uncourteous declamation, he would have act-



ed better for his own respectability at least. Indeed, could I condescend to aim at "victory in dispute" over him, the vituperative language in which he has been pleased to indulge, would furnish the very best proof, that he felt himself deficient in argument. To "misrepresent" him *intentionally*, would be not only the extreme of folly on my part, his paper having gone before the profession, but quite inconsistent with the feelings and principles by which I hope I have hitherto succeeded in regulating my conduct.

He has been pleased to say that I affixed "M.D." to my name! The pages of THE LANCET prove the contrary; and I can assure him, that I should be extremely sorry to take "M.D." as a *substitute* for M.B. Though I admit that, "a rose, by any other name would smell as sweet;" yet, if there be any-thing in a title, it will be conceded, by the intelligent at least, that those who enjoy the privilege of attaching M.B. to their names, need not be anxious to sacrifice it for even Dr. Kennedy's "M.D."

His creditable attempts to prove me "ignorant of anatomy," I am sure that you, Sir, and gentlemen of your cultivated understanding, have read with that pity and forbearance towards him which are always the characteristics of a superior order of intellectual endowment.

Does it follow that, because I said "the murmur from the epigastric arteries can be heard at the mesial line," the vessels themselves must necessarily be situated there? I shall prove even to him, that I at least have not fallen into a mistake of that kind. In THE LANCET, p. 398, col. 1, sixth last line, I laid it down as an incontrovertible fact, that "the resonance extends a considerable way from the point de depart, or centre of radiation." On this principle let us examine my words, of which he so very judiciously (!) attempts to avail himself:—"I was unable to detect the murmur under the mesial line, except when it proceeded from the epigastric arteries, from which it can, in such a case, be easily (mark!) proved to arise." LANCET, p. 397, col. 2, line 24. Now when we move the cylinder from the mesial line outward, the murmur faintly heard at that line gradually increases until we come on the trunks of those vessels, and it is heard with loudest intensity over that trunk only; and this intensity can be traced a little outward and downward towards the internal abdominal ring. So much for his judgment and accurate stethoscopic researches! It is very unlikely that I, not intending myself for a "*mere accoucheur*," should, during my anatomical studies at the admirable schools of this house, and the College of Surgeons, have neglected so important a part of anatomy as the course of

the epigastric arteries. For a mere accoucheur such knowledge I admit may not be requisite, and Dr. Kennedy might be prudent in excluding from the "store-house of his memory" all unnecessary "*lumber*."

Equally unsuccessful shall I prove him to have been in the other, as it were, but yet is not, instance of my ignorance of anatomy. Suppose me unacquainted with the course of the lateral uterine artery, would that instance be sufficient to establish my ignorance of the important parts of anatomy to be learned in the dissecting-room alone, and not from the convenient inspection of casts and plates? But even such inspection might satisfy Dr. Kennedy, that "the lateral uterine artery passes forwards, inwards, and runs between the laminae of the broad ligament to the inferior part of the side of the uterus, where it divides into a number of branches, which anastomose with those of the opposite side, and are all greatly enlarged during pregnancy and disease of the uterus." (Harrison on the Arteries, Vol. II. 2nd edit. p. 82.) This extract may not only silence Dr. Kennedy, but prove to him that when the gravid, or diseased uterus ascends, the murmur may be traced "upward," inward, and forward, towards the mesial line. So much for my "ignorance" of anatomy!!

How Dr. Kennedy can reconcile it with prudence to say, that the feeble murmur, occasionally audible in the "stillness only of night," and to which description of murmur alone did I allude, "is at least 'ten times' louder than the foetal pulsation!" I am perfectly at a loss to account for any other principle than this,—that his stethoscopic ear may be endowed with the rare and enviable faculty of magnifying sounds in a proportion scarcely less remarkable than had that augmenting power possessed, as we are informed, by the celebrated ear of the suspicious tyrant Dionysius, who converted into a stethoscope his ingeniously-constructed prison, sarcastically denominated the "ear of Dionysius;" and thus, like Dr. Kennedy, had recourse to "mediate auscultation," for the purpose of ascertaining murmurs, aye, and the workings of the human heart.

That I have not even attempted to "misrepresent" Dr. Kennedy, will appear evident to any one who is pleased to take the trouble of consulting *The Edinburgh Medical and Surgical Journal* for January, 1831. Where (page 151) it will be found, that he entertained the following opinion:—"Another advantage resulting from the use of the stethoscope in ascertaining the existence of the placental thrill is, that it enables the accoucheur to pronounce on the life or death of a fœtus in utero." (See D. H. Rep. vol. v. p. 267.) Not a word said here respecting the necessity of taking into consideration

"the pulsations of the foetal heart." The editor, forsooth, had some unworthy motive for "misrepresenting him." Again, *Journal*, p. 148, "The placental sound is present in pregnant women *only* when the utero-placental circulation exists, and ceases when the vessels which perform this office become impervious (D. H. Rep. vol. v. p. 242), where, in continuation, Dr. Kennedy says, "a fact which we can ascertain by examining a woman shortly before parturition, when we observe this phenomenon in full energy; and again, when the uterus is empty and perfectly contracted after delivery, or when the foetus, having died in utero, an obstruction in this system of vessels is produced, in all which cases not the slightest vestige of the phenomenon can be discovered." Now mark how inconsistent is all this with what follows, "for the death of the foetus in utero and consequent cessation of the foetal circulation, do not of necessity cause the discontinuance of the soufflet, although they alter its characters." (D. H. Rep. vol. v., p. 244.) This is but one instance of the numerous inconsistencies with which, I must take the liberty of saying, his paper is replete; and which justify me in having said that "I regretted to think it would scarcely stand the test of serious examination." The *Edinburgh Journal*, p. 149, sect. 3, says, "From several cases, two of which are given in detail, the author shows that, when the foetus expires within the womb, and before the placenta is detached, there still continues a thrill, abrupt, however, and short and void of the sibilous lengthened sound by which the placental circulation in its healthy and entire state is distinguished." (D. H. Reports, vol. v. p. 245.)

This is part of the "dangerous theory" I have taken the liberty of combating, without, I hope, condescending to have recourse to unbecoming observations. It will be seen evidently from these extracts, that I have neither misrepresented nor unfairly suppressed any part of Dr. Kennedy's opinions, yet he has been pleased to do me the injustice of insinuating, at least, that I am guilty of both. If at any time I can succeed by facts and arguments in establishing my positions, I am sure the profession will concede that I need not envy Dr. Kennedy that singular species of felicity derivable from undeserved vituperation and assertions, unsupported by a single satisfactory argument.

I deny, in the most unequivocal terms, having said in any part of my paper on "obstetric auscultation," that Dr. Kennedy, at least, considered the placental soufflet as an "unquestionable test of impregnation." When showing that "the presence of a placenta is not necessary for the production of a murmur, such as we ordinarily hear in

the advanced stages of gestation," I used only the expression "test of pregnancy," (p. 399, col. 1, line 5, of *THE LANCET*.) Also in p. 396, col. 2, line 16, I said merely that "the placenta has, in my mind, nothing to do with the production of the murmur, though I know the contrary opinion is confidently maintained by Dr. Kennedy." He will not deny that, under certain restrictions, he does "consider the placental" soufflet a "test of pregnancy." How could I say "unquestionable test," when (*LANCET*, p. 501, col. 1, line 22) the words "their knowledge of practical midwifery in a very questionable shape," taken from his paper, show that the point is levelled directly at him, as *not* considering auscultation the "only unequivocal sign of pregnancy," which (202) he should admit if he deemed even the soufflet an unquestionable test of it. Thus, Sir, can I easily repel even this charge which he (I will not say so disingenuously) would endeavour to fasten upon me."

Having through the middle of his paper (D. H. Rep. vol. v. pp. 241-2-4-6-7-9) laid it down that, the abrupt murmur indicates the death of the foetus in utero, he attempts to support such theory by giving cases illustrative of his views. Then, by way of peroration, and to make certainty doubly sure, he comes to the following conclusion (H. Rep. pp. 268-9)—"We have elsewhere treated of the manner by which the placental soufflet 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." So much for the certainty, and that too elsewhere. Now for the doubly sure. "This," he very sapiently subjoins, "together with the impossibility of detecting the foetal heart's action, particularly if such had been before observed, places the fact of the child's death beyond a doubt." What valuable information this! He then asks,—"Why was the concluding portion of this quotation so cautiously, so unfairly, suppressed?" My answer, I hope, will appear quite satisfactory to the profession. First, because no such concluding portion can be found in the part where he elsewhere treated of the manner, &c.; secondly, he himself did not consider it absolutely necessary; thirdly, even the *Edinburgh Journal* could not find it given by Dr. Kennedy as a necessary adjunct; fourthly, his own friends have been giving out (for I discussed the question with some of them), as an important "discovery," that the "abrupt" murmur (see *LANCET*, p. 495, col. 2, last lines) is sufficient to indicate the death of the child; fifthly, and lastly, he

ought to recollect that I disputed the very point with himself in the presence of some pupils of the hospital (*LANCET*, p. 500, col. 2, l. 15 from bottom); that I then, followed by some of the pupils who were present, took him up to one of the labour-wards to convince him, by an instance, that his theory was not correct; that he still persevered, and made use of these very modest words—"Read my paper, and you will then be convinced!" He cannot deny all this. How then could he assert, "for I have given a case (p. 250, D. H. Rep.) where the sound was not abrupt, although the fetus was dead?" Well, then, the abrupt murmur is no longer to be considered a diagnostic mark of the infant's death!

Now, I hope I shall be able to convince even Dr. Kennedy himself, that he did consider the sound abrupt in the very case he alludes to, for there he says—"No fetal heart's action could be discerned; the placental sound was, however, audible in each iliac region, less abrupt, and approaching more to the natural soufflet than in those above described." Here a clear admission is implied, at least, that the murmur was abrupt, but less so; was not natural, but approaching more to the natural than in those above given; and that he himself considered it abrupt will appear evident from the following observations relating to this very case too (pp. 250-1, D. H. Rep.) "From these observations, let it not, however, be supposed, that the placental sound is always observable, even of this modified character." So much for the applicability of his logical crotchets, the *suppressio veri* and the *assertio falsi*!

With respect to the soufflet he found produced by an enlarged liver (D. H. Rep. vol. v. p. 26), I thought it really undeserving of attention; first, because he does not even say in what part of the abdomen it occurred; secondly, he, without adding even a qualifying term, says, "it resembled the placental soufflet," which, in the next page, he admits to be "quite distinct in its nature, and easily recognised by persons at all conversant with it, resembling a sound quite distinct, and one easily recognised." What consistency! I must be pardoned for taking the liberty of dissenting from two assertions of his in p. 267, D. H. Rep.; first, that "the placental soufflet can be heard indifferently over the abdomen;" secondly, "that the soufflet produced by a tumour can be removed by altering the position of the patient." For in Corrigan's case (*LANCET*, p. 399, col. 1) the soufflet continued, no matter what position the patient was put into. In the same case there was heard by me and others, as I stated, "the self-same, identical description of murmur, which usually occurs in the ad-

vanced stages of pregnancy." I am, therefore, warranted in withholding my assent to Dr. Kennedy's assertion, that "the placental soufflet is in its nature quite distinct and easily recognised;" and, indeed, one of two consequences must follow from this assertion of his, namely, that either his opinion is incorrect, or the soufflet placental, as he calls it, must be considered as "infallible" proof of pregnancy, "if it be owing to the presence of a placenta, and if it be of such a quality as to be, in its nature, quite distinct and easily recognised by persons at all conversant with it." For, if it be "owing to the presence of a placenta," as he thinks, but I take leave to deny, we can have it only where there is a placenta, and, consequently, pregnancy; and, if "it be in its nature quite distinct and easily recognised," as he says (p. 267), I should like really to know how he can reconcile it with right reasoning to deny, that it, when heard, should not be considered an "infallible test" of utero-gestation. But, talented and accurate logician as he is, I apprehend he will find it rather difficult to extricate himself from such a dilemma. It must strike every rational man, as a manifest absurdity, that this soufflet should be designated "quite distinct in its nature," and also placental, yet be denied as an "infallible test of utero-gestation." He, however, denies it! and is found to have expressed himself to the following effect (vol. v. D. H. Rep. p. 257): "When a perfect placental soufflet exists in any part of the abdominal tumour (particularly if the fetal heart's action also can be detected), we may pronounce the woman pregnant." Suppose the fetal heart cannot be heard, and that, however, "a perfect placental soufflet" is audible, it must, according to his theory, appear to any one, that "we may pronounce the woman pregnant."

I cannot condescend to notice his observations on the expression "ninety-nine in a hundred," a very general phrase in our language, and never used in its strict, literal sense. Though Dr. Kennedy evidently prides himself on the perspicuity of his style, for he is astonished how even I could misconceive his meaning, yet he is, I am sorry to think, often contradictory, unclear, and unhappy in his mode of expressing that meaning, as in the following (p. 496, col. 2, of *THE LANCET*):—"I would, however, and with justice, have been to blame, as well upon my own account as on that of the profession generally, had I allowed such a tissue of misrepresentation to remain uncontradicted. Having now done so, I must," &c. Having done what? Why, having allowed such a tissue to remain uncontradicted. Besides; a tissue uncontradicted!

It is true I cannot prevent him from—

"doubting whether I am capable of recognising the phenomenon of which I treat;" but he ought to have recollected, that I am, however, the first in these countries, at least, who detected, by means of auscultation, the existence of twins in utero (p. 232 of THE LANCET);—what he, with all the facilities afforded him by one of the most splendid hospitals in Europe, was never yet able to accomplish; else we should have heard something of that "discovery" also—"Hinc illæ lachrymæ," *inanesque ululatus in auras!* I am sure it will be conceded by the profession, that he has done me great injustice even respecting my much-valued friend Dr. Clinton, whom, as will be seen by reference to p. 400 of THE LANCET, I have not even attempted to identify with any one of my views and statements, further than by merely saying, I felt gratified having my opinion coincide with his, as far as related to the "site of the soufflet."

Thus far, Sir, have I endeavoured to meet candidly, fairly, and fully, I hope, the unmerited imputations thrown out against me in Dr. Kennedy's letter; and I shall take leave to avail myself of this opportunity of assuring him, that should I, in future, have occasion to speak of the contents of his *valuable* paper, it shall be done in the words, but not in the spirit, of the sarcastic Roman poet—"Euge, omnes, omnes, bene miræ eritis res!"

Believe me, Sir, sincerely yours,

DAVID C. NAGLE.

33, Trinity College, Dublin,  
January 18th, 1831.