and slowly moved, and maintaining in all these respects a tolerably uniform character, we may assume that the heart is not only enlarged, but that its walls are thickened. But if, after exhibiting this diffused heavy impulse, the heart fall back with a jog as it were, and the impulse itself be diffused over a large area, with a dulness on percussion, especially to the left of the sternum, we may further assume that the above diseased conditions are somewhat advanced. Extension rather than force appears to be the measure of the amount of diseased enlargement and thickening. There may be a considerable augmentation of impulse without enlargement; but an impulse diffused over a large area and without sharpness in the blow, so that it rather gives the impression of a deep-seated, extended, and perhaps feeble beat, and wanting in a clear definition of its extent, is a sure indication of thickening and enlargement of considerable extent. It is said that, when the impulse presents the above characters as regards force, but is limited to an extent less in area than is natural, the presence of a simply concentric hypertrophy is indicated. This is, however, a very doubtful condition for the heart to exhibit; and if it really do occur, is only of so rare occurrence, that it is the lot of but few practitioners to have met with and determined its existence. If the impulse and dulness on percussion be projected over an area above as well as below the left nipple, and at the same time the beat be dull and heavy, we may assume that there is an hypertrophied condition of the left ventricle; and, if this impulse and dulness are further traceable to the left of this vertical line, that the hypertrophy of this ventricle is far advanced, and involves a considerable amount of structural change, inasmuch as an hypertrophied condition of the walls of this cavity first induces vertical enlargement, and then, as it progresses, horizontal enlargement.

If the impulse be of the above character, and yet be not projected to the left of the nipple, while a dulness on percussion is, at the same time, traceable behind the lower part of the sternum and projects itself into the epigastrium, an enlargement of the right ventricle is indicated. Disease in this ventricle widens horizontally. This enlargement may be due solely to hypertrophy, more commonly however to hypertrophy with dilatation; but, in either case, it is necessary to bear in mind that, by elevating and thus mechanically displacing the left ventricle, an erroneous conclusion as to the existence of an hypertrophied condition of this ventricle may be inferred. In such case, the dulness traceable below and above the nipple does not necessarily indicate enlargement of the left ventricle. Nevertheless, it is so rarely the former is dissociated from the latter, that practically there is scarcely room for the doubt.

It has been stated that when hypertrophy of the heart is clearly indicated, there are no certain means of distinguishing which cavity is exclusively its seat; still, the existence of a basic dulness on the one hand, and on the other of a dulness projected to the right of and below the sternum should, as regards the condition of the ventricles, have its weight in our means of diagnosis. The indications of thickening and distension of the auricles are not so certain, but it will be borne in mind that the auricles are, generally speaking, only so diseased when ventricular disease is largely developed, or there exists extensive inefficiency in the mitral or tricuspid valves. If the dulness on percussion and the impulse be traceable, as above described, over a diffused area—whilst the force of the impulse is neither massive nor heaving, but abrupt, sharp, and rising as it were to the surface, presenting both to the hand and to the eye an undulatory vibration, and this diffused over nearly the whole area of percussion-dulness, now tolerably uniform in rhythm, now presenting much rhythmical disturbance, for the most part uniform in force, but on slight excitement statically increased, and thus presenting the phenomena of paroxysms of palpitation, perhaps of rare, perhaps of frequent recurrence—the presence of dilatation with hypertrophy may be inferred.

If the area of dulness be projected downwards as well as laterally, and the apex-beat be ill-defined and lost in a feeble weak impulse of an undulatory character, and which to the eye may appear strong and violent, and traceable over the whole area occupied by percussion-dulness, but offering to the pressure of the hand only a slight resistance, being, in fact, really a weak impulse, though, from impinging immediately on the surface, apparently a violent one—it may, under these circumstances, be assumed that the heart is mainly enlarged by dilatation, and that its walls are only slightly, if at all, hypertrophied.

Whether there be or be not percussion-dulness over an enlarged area, if the apex-beat be very distinct, and then diffuse itself into a superficial undulation obvious both to the eye and to the hand, giving the impression of a mass moving immediately below the surface, and in some cases dimpling on the diastole, we may suspect, whether the heart be enlarged or not, that it is adherent to the pericardium, and perhaps, through it, to the pleura also. If there be at the same time a bulging of the cartilages of the fourth and fifth ribs, this latter con-

dition most probably coexists. This præcordial fulness or bulging may also exist in hypertrophy with dilatation, though there be no pleural nor other adhesions; but it is never met with in cases of dilatation only.

[To be continued.]

ON MECHANICAL DILATATION OF THE CERVIX UTERI.

By J. MATTHEWS DUNCAN, M.D.,

Physician to the Royal Maternity Hospital, and Lecturer on Midwifery in the School of Medicine, Edinburgh.

THE following remarks are written as an acknowledgement of and response to the various comments on my paper recently published in this JOURNAL (November 9 and 16, 1872). I feel much indebted to my gynæcological and obstetrical brethren for their criticisms, and recognise their importance. In dealing with them, I shall suppose the reader to be acquainted with the statements of the various writers: this, in order to avoid tedious length.

Dr. Aveling's objections to the use of the metallic bougie are truly only statements of difficulties; and these difficulties every gynæcologist overcomes in daily practice. Besides, the difficulties or objections apply to every mode of dilating the cervix quite as much as to that by a series of metallic bougies. And again, if surgeons were governed by difficulties, as Dr. Aveling thinks gynæcologists should be, they would never use an urethral bougie or an exploring probe.

When Dr. Aveling comments on my use of great force by the bougie, he forgets that it is a rare application, and that I have not said, in the paper referred to, under what circumstances I think it justifiable. Besides, he forgets that he himself, using tangle-tents, applies force enormously great, and greater, though in a different way, than any I ever mention as exercised by bougie. When he says that he has always learned and taught that no force should be used in passing the uterine sound, and when he speaks of doing work by art, not by force, he is at least unintelligible. I can understand work being done by a small force, or by art and force, but not otherwise; and my object in the paper was to ascertain the amount and discuss the use of force.

His comment on the important element of time is ingenious, but not convincing. A surgeon who would dread leaving a bougie in the urethra for a day, would not hesitate to leave it there for a few minutes; and he would contrast the few minutes with the day; not (as Dr. Aveling ingeniously but erroneously does) "the few minutes multiplied into a series of daily applications" with the day. I am sure that in gynæcological practice time is very important, and that the safety of the bougie plan of dilatation is mainly owing to the shortness of the period of its application; a safety that is illustrated by a thousand analogies in surgery and medicine.

Dr. Atthill makes the same mistake, in using the term force, as is made by Dr. Aveling. If "gentle continuous pressure" and "no force" be not inconsistent and incompatible statements, I must acknowledge ignorance of the meaning and use of the simplest scientific terms. How gentle continuous pressure can be not a force, I cannot conceive.

When Dr. Atthill says that dilatation of the cervix uteri by tangle-tents has in his practice been followed by no unpleasant symptom, I can only say that there is here something to explain. The records of gynæcology contain superabundance of direct evidence of their often causing inflammation, sometimes even unto death. Indeed, this is almost demonstrated by his own statement, that the use of graduated metallic bougies often causes serious and even fatal consequences. They should be, and are, I believe, at least as innocent as tangle-tents. But both methods are in my opinion greatly, very greatly, more dangerous than that which I employ, using graduated metallic bougies. In my method the bougie is not left above a few minutes; the danger of the ordinary mode of using graduated metallic bougies arises from leaving them for hours or days in the uterus.

Dr. Atthill appears to think me blamable or unwise in recommending a method of treatment of disease by dilatation, which directly violates the law which Nature follows in dilating the healthy uterus. But I cannot see the force of the objection or criticism. To a great extent I agree with the distinguished pathologist, who recently described Nature when exemplified in disease as leading or enticing to the coffin, not towards cure; requiring from the physician opposition or resistance, not assent or imitation. I am not aware that gynæcologists, obstetricians, surgeons, or physicians acknowledge any such canon of treatment as is implied in the observations of Dr. Atthill.

Dr. Henry Bennet has so utterly misunderstood, and consequently misrepresented me, that I shall not enter on his remarks, except to

make the two following statements. I never anywhere said or suggested that a force of "four pounds" was required to pass a probe in a healthy woman, nor a force of even half a pound. Dr. Bennet's assertions as to the state of the healthy *os uteri*, *externum* and *internum*, are, I am convinced, erroneous and inconsistent with anatomy, physiology, pathology, and daily experience. It is well-known that some gynaecologists speak and treat as if every woman suffering from dysmenorrhœa or sterility had a strictured cervix; but it is not necessary, in order to account for such proceedings, to resort to Dr. Bennet's dictum that the *os uteri internum* is normally closed during life, except at certain times.

These remarks render it unnecessary for me specially to consider the observations of Dr. Boulton; and those of Dr. Greenhalgh raise no question upon which I am called to make comment.

I thank each of the gentlemen above referred to for their valued criticisms; and I end by saying that I believe the treatment of spasmodic dysmenorrhœa by mechanical means appears to me, at present, to be best effected by the use of a graduated series of metallic bougies, applied as I have elsewhere described—never at any time allowed to remain in the uterus above a few minutes. [See *Edin. Med. Journal*, May 1872.]

ON PALSY OF THE VOCAL CORD FROM INTRACRANIAL SYPHILIS.

By J. HUGHLINGS JACKSON, M.D., F.R.C.P.,

Physician to the Hospital for the Epileptic and Paralysed, and Physician to the London Hospital.

I WISH briefly to draw attention to the occasional nervous origin of syphilitic aphonia, if such an expression may pass. Suppose a patient, who is manifestly the subject of syphilis, becomes aphonic. It would be unsafe—especially if he had any evidently nervous symptoms—to conclude, without looking into his larynx, that he had syphilitic disease of that organ itself. It would be as unjustifiable as concluding, without ophthalmoscopic examination, that his blindness, if he were blind, too, was owing to syphilitic changes in the eye itself. There is another possibility; the fact is that, in some cases of "syphilitic aphonia" there is no other abnormality discoverable in the larynx than paralysis of one vocal cord. In these cases the palsy may safely, in a person presenting outward signs of syphilis, be put down to syphilitic disease affecting the rootlets of the eighth nerve. The recognition of this possibility is obviously of moment. For there is as much practical difference betwixt affection of voice from syphilitic laryngitis and affection of voice from syphilitic disease of the eighth nerve, as there is betwixt affection of sight from syphilitic iritis and affection of sight from paralysis of the third nerve.

I am convinced that, in physicians' practice, aphonia from intracranial syphilis is not exceedingly uncommon; at any rate, it is so far from being very rare, that we can be practically certain that aphonia in a syphilitic patient is owing to syphilitic changes in the larynx itself. And if the patient have paralysis of any cranial motor nerve—for example, the third nerve—the question surely will arise automatically, "Why should not this patient's aphonia also be due to affection of a nerve-trunk as well as his double vision?" "Why should not the eighth nerve suffer from syphilis as well as the third or fifth?" For it is as certain that the eighth nerve supplies the muscles of the vocal cords, as it is that the third supplies the muscles of the eyeball.

I have seen several cases of paralysis of the tongue, palate, and vocal cords from, as I believe, intracranial syphilis; but I will refer only to two cases of palsy of the vocal cords in which *post mortem* examination disclosed syphilitic disease involving, among other parts, the eighth nerve. I would first remark, however, that in these cases, and in all other cases of palsy of the intrinsic muscles of the larynx which I have published, the larynx has been examined for me by my colleague Dr. Morell Mackenzie.

In the *London Hospital Reports*, vol. iv, 1868, p. 314, I have reported the case of a man who had paralysis of the *portio dura*, of the tongue, and palate and vocal cord, all on the left side. Dr. Morell Mackenzie examined the larynx, and established for me two things: (1) that the left vocal cord was paralysed; and (2) that there was no other sort of affection of the larynx. The patient subsequently died hemiplegic from blocking of cerebral arteries. The points of this case which I wish to make prominent are (1), that there was, among other lesions, intracranial syphilitic disease involving all the nerves of the medulla oblongata on the left side, the eighth nerve, of course, among them; (2) that the larynx was found on *post mortem* examination, as Dr. Morell Mackenzie had predicted, free from local syphilitic disease. Mr. Frederick Mackenzie dissected the larynx, and only atrophy of the left crico-arytenoid muscle.

The other case (*op. cit.*, p. 318) is (in so far, I mean, as it illustrates the occurrence of aphonia from intracranial syphilis) so like the one above mentioned, that I will only say of it, that Dr. Morell Mackenzie found, as in the other case, palsy of one vocal cord, and no other affection of the larynx itself; and that Mr. Frederick Mackenzie reported of his dissection of the larynx—"except atrophy of the intrinsic muscles on one side, nothing of any importance was found."

I will not enter into the consideration of purely physiological topics. Bernard's experiments on the spinal accessory and pneumogastric nerves, and on the different sources of the motor fibres which the trunk of the pneumogastric contains for the double set of movements—vocal and respiratory—of the larynx, are well known, and may be read in any work on physiology. I have given a short account of this subject in the *London Hospital Reports*, vol. i, 1864. (The two cases mentioned in this paper are recorded in the fourth volume.) At page 363, vol. i, 1864, I give a brief account of Dr. Lockhart Clarke's researches on the relations of the nuclei of the spinal accessory and lingual nerves.

A CONTRIBUTION TO SCHOOL HYGIENE.*

By R. LIEBREICH, Esq.,

Ophthalmic Surgeon to St. Thomas's Hospital.

THE contributions to school hygiene which I shall put before you do not refer to questions which would come under your consideration as a matter of course. I rather wish to draw your attention to a subject which hitherto has not been considered as belonging to your domain, but the close connection of which with general hygiene it seems to me most important to prove; I allude to the lighting of the class-rooms, and the shape and position of the tables and seats. At first sight, it might appear that this could only refer to the special hygiene of the eye. But this is not the case. Indeed, badly lighted rooms and improper school-furniture spoil the eyes and increase short-sightedness. But this is not the only bad effect. The lungs and viscera become compressed, the spine and shoulder-blades are kept out of their normal position, by the constrained attitude in which the youthful body, at the very time of its growth and development, is kept for hours every day. Thus the way is prepared for the development of chronic diseases; and this must not only considerably impair the health of individuals, but also tend to promote physical degeneracy, and deteriorate the vigour and *physique* of the nation.

If the bad attitude of the children in schools could be only ascribed to want of attention on the part of the masters, and to carelessness on the part of the children (as is generally assumed), then, indeed, you would have nothing to do with the matter. It is, however, easy to prove that, with the present arrangements, it is impossible for the children, from physiological as well as from anatomical reasons, to remain straight for any great length of time, but that they could be enabled to do so by a proper distribution of light, and by well adapted school-furniture.

The proper light is most easily obtained if the class-room be of an oblong shape; the windows being in one of the long sides, and the tables arranged parallel to the short walls, so that the light may fall from the left side. The desk of the master ought to be placed near the short wall towards which the scholars look.

Light coming from the right hand is not so good as that from the left, because the shadow of the hand falls upon that part of the paper at which we are looking. Light from behind is still worse, because the head and upper part of the body throw a shadow on the book; but the light that comes from the front, and falls on the face, is by far the worst of all. In the first place, it does not attain the object desired; and next, it is most hurtful to the eye. The object is to make the fully illuminated faces more visible to the master; but the children, instinctively desirous of avoiding the unpleasantness of the full glare, assume all sorts of positions which turn their faces from the master. In reading, they turn the head round the vertical axis, generally towards the right, in order to let the light fall on the book, which, when held straight before them, is completely in shadow; while in writing, or in reading (the book being on the table), they bend their heads as low as possible, in order to shade their eyes by the projection of the forehead. In this way the faces are much less visible to the master than if they were held upright and illuminated from the left side; and if, according to the regulations of the Committee of Council, the light also fall full upon the face of the master, he will be entirely prevented from seeing them. This method of lighting the room is very injurious to the eye, because,

* Read before the Association of Medical Officers of Health.